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THE PSYCHOLOGICAL REVIEW.

SHORT STUDIES IN MEMORY AND IN ASSOCIATION FROM THE WELLESLEY COLLEGE PSYCHOLOGICAL LABORATORY.

COMMUNICATED BY MARY WHITON CALKINS.

I. A STUDY OF IMMEDIATE AND OF DELAYED RECALL OF THE CONCRETE AND OF THE VERBAL.

Modern methods in education, from the kindergarten to the laboratory, emphasize the fact that the concrete is far more readily memorized than the verbal. To bring this fact to an experimental verification and to an exact expression, Mr. E. A. Kirkpatrick[1] studied the facility with which students remembered words pronounced to them, words read by them, and concrete objects shown to them. The visualized words were written, one after another, on a blackboard, and the objects—as nearly as possible of the same size and impressiveness—were shown, one by one. The subjects of the experiment were primary, grammar school and high school pupils, and 150 college students, of whom 50 were women.

It has seemed worth while to repeat this experiment with 50 college students, all women, not only in order to add to the data, but also to secure greater uniformity in the conditions. For this purpose the pictures of concrete objects and words were projected upon a screen by the oxy-hydrogen lantern, and the times of exposure were regulated by Mr. Lough's electrically actuated pendulum. The intervals of the words pronounced were similarly timed. The stimuli, actually used, differing from Mr.

[1] PSYCHOLOGICAL REVIEW, I., p. 602.

Kirkpatrick's in a few particulars only, to secure greater similarity or greater concreteness, are the following:

Words pronounced: Chair, lock, fan, slate, rug, box, watch, tub, cloud, stamp.

Words shown: Door, foot, cane, horn, sail, bowl, ring, fork, nail, lamp.

Pictures shown: Scissors (in 19 cases, box),[1] book, brush-broom, inkbottle, clock, hat, knife, candle, flat-iron, vase.

The grouped results of the experiment, compared with Mr. Kirkpatrick's figures for his college women, are these:

		Auditory Works.		Visual Words.		Pictured Objects.	
		Remembered.	Remem. *in order.*	Remembered.	Remem. *in order.*	Remembered.	Remem. *in order.*
Immediate Recall.	K.	**72.** %		**72.8**		**85.6**	
	Wel.	**84.2**	66.3	**89.8**	78.4	**93.5**	75.2
Delayed Recall.	K	**17.9**	12.9	**32.2**	22.4	**54.4**	54.4
	Wel.	**34.9**	14.3	**48.2**	24.6	**74.5**	32

These figures abundantly justify the general conclusion of Mr. Kirkpatrick that *the superiority of concrete to verbal, as memory material*, is under-estimated, rather than over-emphasized, even by its most strenuous upholders. These results, like his, show that *this difference is much more marked in the case of delayed, than in that of immediate recall;* he has three times as many instances of concrete as of auditory verbal recall, while the Wellesley subjects, in spite of a much higher per cent. of verbal recall, still remember twice as many concrete objects. The instability of verbal as compared with concrete memory is thus very definitely indicated.

The summary discloses also certain divergences from Mr. Kirkpatrick's results. The Wellesley per cents. of recall are uniformly higher, perhaps because of the more careful and even conditions of the experiment. The distinction between verbal and concrete memory, important as it is, is in every case less than in Mr. Kirkpatrick's records. Finally, the Wellesley conclusions do not substantiate in any degree the conclusion (*op. cit.*, p. 605) that the number of concretes 'recalled' and

[1] The use of this picture was an oversight, since the word 'box' belongs to the auditory series. The 19 cases in which it appeared in both places were left out of account in calculating per cents.

the number 'recalled in order' would be, under ordinary conditions, 'practically the same.' On the other hand, only one-half, roughly speaking, of the number actually remembered are recalled in their exact position and *nearly as large a per cent. of visual words as of concretes is thus recalled in order.*

The study of the individual records, though it corroborates in a general way Mr. Kirkpatrick's statement that 'individual differences are marked,' nevertheless very strongly emphasizes the underlying similarities. The *'delayed recall' is surpassed by the 'immediate' for every subject*, except in eight cases of concrete memory and in two of verbal memory, in which there is no difference. By five subjects also the order of concrete objects is as well or better remembered, while by four the order of words is as well or better recalled, after the two days' interval. By all the others words and pictures immediately recalled are remembered better, even in their order.

The distinction between concrete and verbal memories corresponds, also, with that indicated by the massed results. Forty-seven subjects remember pictures after a two days' interval better than they recall auditory words, forty-four remember pictures as well or better than visual words, and there are only five subjects whose delayed memory for pictures is surpassed by either sort of verbal memory. There is less difference, of course, between the concrete and the verbal memory in the case of immediate recall, yet there are only nine subjects whose visual verbal memory surpasses their memory for concretes and only eight who remember more of the words pronounced immediately after these are given. No subject shows a superior verbal memory throughout, but there are two whose memory for visual words is stronger, both in immediate and in delayed recall.

The proportion alters when one considers the memory for words and for pictures in their exact position. Here the number of subjects whose immediate memory for visual words is strongest equals the number of those who remember best the order of concretes; and even after two days' interval, fourteen subjects recall better the order of the visual words than that of the pictures. Doubtless this relative advantage of the visual

verbal memory is explained by the habit of visualizing a column or a line as a whole.

Finally, the individual record shows clearly that *the memory for visual words is significantly stronger than that for words pronounced and heard.* One-half of the subjects recall more visual than auditory words immediately after they have been shown, while only one-fifth remember more auditory words and three-tenths show no difference; in the delayed recall, moreover, three-fourths remember visual words better than auditory, and only one-seventh have a better memory for the auditory words.

These distinctions are included in the summary which follows, whose figures refer to the records of subjects,[1] not to the per cents. of words and pictures recalled.

Concrete Memory Compared with Verbal.

a. IMMEDIATE RECALL.

1. Total Recall.

The Concrete Memory.

Stronger than		Equals		Weaker than	
Verb. *Aud.*	Verb. *Vis.*	Verb. *Aud.*	Verb. *Vis.*	Verb. *Aud.*	Verb. *Vis.*
25 subjects	20	16	20	8	9
13 (same subjects)		8		5	

2. Ordered Recall.

27	21	13	7	9	21
13		2		7	

b. DELAYED RECALL.

1. Total Recall.

The Concrete Memory.

Stronger than		Equals		Weaker than	
Verb. *Aud.*	Verb. *Vis.*	Verb. *Aud.*	Verb. *Vis.*	Verb. *Aud.*	Verb. *Vis.*
47	37	2	7	0	5
36					

2. Ordered Recall.

35	27	5	8	9	14
25		1		5	

[1] Comparisons involving reference to concrete memory are based on the records of 49 subjects; with this exception, there are 50 subjects throughout.

Visual Verbal compared with Auditory Verbal Memory.
(Total Recall.)

a. IMMEDIATE. *b.* DELAYED.

The Visual is			The Visual is		
Stronger.	Equal.	Weaker.	Stronger.	Equal.	Weaker.
24	15	11	36	7	7

A better comparison of the differences of the individual records is afforded by still another condensation of them:

Immediate Recall.

Percentages of Words Recalled.[1]	Auditory Words.		Visual Words.		Pictured Objects.[2]	
	Remembered by	Remem. in order, by	Remembered by	Remem. in order, by	Remembered by	Remem. in order, by
100 %	**14**	9	**17**	15	**28**	14
90 (or 89) %	**16**	7	**20**	10	**12**	3
80 (or 78) "	**7**	5	**8**	7	**8**	13
70 (or 67) "	**7**	9	**5**	6	**1**	5
60 "	**4**	2	**0**	2	**0**	2
50 (or 55) "	**1**	2	**0**	5	**0**	4
40 (or 44) "	**1**	7	**0**	2	**0**	6
30 (or 33) "	**0**	6	**0**	3	**0**	2
20 (or 22) "	**0**	2	**0**	0	**0**	0
10 (or 11) "	**0**	1	**0**	0	**0**	0
0 "	**0**	0	**0**	0	**0**	0
			Delayed Recall.			
100 %	**0**	0	**2**	2	**2**	1
90 (or 89) %	**1**	0	**5**	1	**10**	1
80 (or 78) "	**2**	1	**1**	1	**20**	1
70 (or 67) "	**4**	0	**3**	1	**7**	1
60 "	**2**	1	**6**	0	**1**	4
50 (or 55) "	**6**	0	**8**	2	**5**	4
40 (or 44) "	**8**	5	**10**	4	**2**	6
30 (or 33) "	**9**	3	**5**	8	**2**	8
20 (or 22) "	**6**	7	**7**	8	**0**	8
10 (or 11) "	**5**	12	**2**	13	**0**	10
0 "	**7**	21	**1**	10	**0**	5

Of these subjects, immediately after the series were given, *two* recalled all the words and pictures in their exact order, while *four* others remembered all irrespective of order; *six* remembered all the pictures and one of the verbal lists in exact order, while two more remembered these out of their order;

[1] The per cents. of auditory and of concrete recall are reckoned for nineteen subjects, upon the basis of a series of nine stimuli only. This is required by the careless use of 'box' both as verbal and as picture-stimulus, so that it obtains an unfair emphasis.

[2] There were but 49 subjects in the experiments with pictured objects.

and finally *three* subjects remembered both verbal series. No one remembered more than one entire series after the two days' interval.

The following table compares the per cents. of recall with reference to the position of the various stimuli in the series:

Early Recall.

Position in Series.

Stimuli:	1	2	3	4	5	6	7	8	9	10
Concrete.[1]	**100**(100)[2]	**98**(98)	**100**(98)	**100**(90)	**88**(73)	**96**(76)	**94**(65)	**83**(48)	**83**(47)	**93**(65)
Words (Vis.)	**100**(98)	**100**(98)	**100**(92)	**90**(82)	**90**(82)	**92**(78)	**92**(72)	**80**(66)	**78**(58)	**76**(58)
Words (Aud.)	**96**(92)	**90**(84)	**92**(78)	**72**(58)	**86**(56)	**90**(80)[2]	**80**(48)	**80**(46)	**72**(52)	**86**(74)
			Late Recall. Position in Series.							
Concrete.[1]	**87**(63)[2]	**83**(53)	**78**(35)	**80**(38)	**63**(25)	**67**(16)	**78**(16)	**76**(22)	**63**(24)	**76**(41)
Words (Vis.)	**72**(54)	**62**(36)	**44**(30)	**40**(24)	**46**(20)	**36**(16)	**42**(18)	**38**(14)	**46**(14)	**56**(20)
Words (Aud.)	**54**(34)	**32**(16)	**40**(18)	**16**(6)	**44**(16)	**45**(62)	**30**(8)	**38**(12)	**38**(14)	**16**(10)

No very startling conclusions emerge from these figures. In all cases the first of the series, closely followed by the second, is best recalled, while the middle ones are least accurately remembered. It is more surprising to find that the last of the series has so little advantage; in the delayed recall of auditory words it is even one of those least well remembered.

Many of the errors are suggestive of the methods of memorizing employed. Some of these show inadequate verbal memory, as when vowel combinations are correctly remembered but combined with incorrect consonants. 'Stool' for 'broom' is an instance of this sort. Certain mistakes in order must be laid to the door of habitual association: so 'ring' is recalled just after 'hand,' and 'inkbottle' after 'pen.' Most instructive of all, perhaps, are those incorrect equivalents, like 'light' for 'lamp,' 'mat' for 'rug' and 'boot' for 'shoe,' which show that memory for words is itself often concrete, that is, that not the words themselves but the suggested images are remembered.

II. The Tendency to Mental Combinations.

There is a sense in which all actual contents of consciousness are complex, not simple, since the 'psychic element' is an

[1] Only 49 cases to be considered. (Cf. foot-note, page 454.)

[2] Cases involving the stimulus 'box' are not considered. (Cf. foot-note, on page 452.) The numbers in parentheses state the cases of *ordered recall*.

acknowledged abstraction. The complexity of the percept is, however, no part of its content, but is rather a characteristic later observed by the psychologist; the percept itself is one and simple to the immediate consciousness. But besides these unobserved, these analyzable but unanalyzed perceptual complexes, there are conscious groupings and combinations whose relatedness forms part of their very content. Such are the widening memory-picture, the growing ideal of imagination and, most explicitly, the judgment or syllogism, holding within its realized unity the discriminated and balanced parts which compose it.

The prominence of the disposition to group even objectively distinct and separate facts of consciousness is the subject of a simple experiment whose general plan[1] is to present to the subject unconnected stimuli, and to note carefully the tendency to combine even the most diverse of them. The study is based upon the records of 50 students, in 638 tests. The stimuli were of three sorts: words spoken, words printed and projected by an oxy-hydrogen lantern upon a screen, and pictures of concrete objects exposed in the same manner. From twenty to thirty subjects were present at each experiment; the average number of tests was 12.7 (excluding those for practice), and no one of the subjects assisted in less than eight nor in more than fifteen. The spoken words were pronounced *staccato* fashion, with a momentary pause between them; four seconds of silence followed, concluded by the direction 'write;' the subjects then wrote out in full the train of imagination which had filled the silent interval. In the case of the visual stimuli, two disconnected words or pictured objects were thrown upon the screen for six seconds, and the direction to record the suggested images followed immediately. Lists of the words and of the pictured objects are given below. It will be observed that a uniformity of suggestiveness is secured, so far as may be, since all refer to simple, concrete things.

The intervals and the times of exposure were accurately marked by an electrically actuated pendulum devised by Mr. James E. Lough, which was connected with a telephone or with the shutter of the lantern. Both the visual words and the

[1] Elaborated from a suggestion of Professor Münsterberg.

pictures of objects were separated by so wide an interval that they appeared entirely unrelated.[1]

The records thus obtained have been carefully studied with constant recourse to the subjects themselves, in cases of doubtful interpretation, and the percentages given are based on figures which have been carefully verified by the writer. In more than one-fifth of the possible cases, the tendency to combine even the radically unconnected plainly appears. The words

Words Pronounced.		Words Visual.		Pictures of Objects.	
Page	Hook	Hair	Post	Oil can	Pin cushion
Wall	Tree	Rope	Leaf		(small)
Fog	Rat	Card	Roof	Shell	Cone
House	Nose	Bird	Cuff	Apple	Large spool
Sash	Ear.	Face	Bush	Tumbler	Purse
				Bottle	Large key.

'rope—leaf,' for instance, suggest to one subject (M.W.C., CV,2) a "scene * * * several centuries back in a warm country where vegetation is luxuriant. I saw an old house and there was a trellis covered with a mass of leaves which extended to a second story window. At the window was a maiden * * * talking to her lover who stood below. A stout rope was hanging out of this window falling down over the leaves and reaching to the ground."

The exact numbers[2] of these combinations from separated stimuli are the following:

With words heard: 36 (and 3 doubtful), or 18.8 per cent., of 207 possible cases.

With words seen: 68, or 33.5 per cent., of 203 possible cases.

With pictures seen: 34 (and one doubtful), or 15.3 per cent., of 228 possible cases.

In all: 138 (and 4 doubtful), or 22.2 per cent., of 638 possible cases.

[1] The words were made by raised, gummed letters fastened directly to glass slides; the outline-pictures, drawn by Miss Julia D. Randall, were photographed upon glass slides by Mr. Horace Sumner, mechanician of the Harvard Physiological Laboratory. The scale of drawing was, of course, the same throughout.

[2] Six cases are excluded in which more than one such combination occurred in connection with the same stimuli.

These figures do not, however, adequately represent the tendency to mental grouping disclosed by the conditions of this experiment. For, besides these cases of the combination of the immediate percepts, or of the images at once suggested, there are 55 in which the second percept connects itself with one of the later images following upon the first stimulus, or in which two such secondary images are combined. For instance, the word 'rope' of the pair 'rope—leaf,' suggested to one subject (M. E. C., CV, 2) "'rope of sand;' a rope lying on the sand with one end frayed out." The word 'leaf' then suggested "a rose-leaf on a rosebush in the yard of a house I lived in." The case of combination which followed grouped together these later images in the mental picture of "the same rope on the grass near the rosebush."

A complete summary,[1] therefore, of the mental combinations directly and indirectly connected with the stimuli given, is as follows:

With words heard: 60 (and 5 doubtful), or 31.4 per cent., of 207 possible cases.

With words seen: 79 (and 1 doubtful), or 39.4 per cent., of 203 possible cases.

With pictures seen: 49 (and 3 doubtful), or 22.8 per cent., of 228 possible cases.

In all: 188[1](and 9 doubtful), or 30.8 per cent., of 638 cases.

The occurrence of these combinations in all forms of the experiment, visual as well as auditory, verbal as well as concrete, indicates a general characteristic of conscious experience. In more closely comparing the different results, those of the auditory experiments must be set aside, since the first word pronounced tends often to establish itself so firmly, that its associated images are proof against the intrusion of the second word, which has, therefore, no chance to be grouped with the first. (As a matter of fact, in the 207 auditory experiments, there are 190 associations with the first word, and only 148 with the second.) Yet even in the auditory experiments, and still more clearly in the visual tests, which avoid this difficulty by

[1] It should be noted, however, that in two-fifths of the 55 added cases of secondary combination, the images connected do not differ so radically as the original stimuli.

the simultaneous presentation of the separate stimuli, a comparison of the two types of experiment shows that the words far more often than the pictured objects combine in suggestiveness. The images associated with words are evidently more vaguely localized, and thus more readily transposed and combined, than images from objects definitely perceived.

The fifteen distinct experiments conform closely to the type, showing only unimportant deviations from the average. Individual variations, on the contrary, are so marked as to forbid any disposition to treat these figures as an accurate estimate of the general frequency of this involuntary grouping of conscious contents. Of the fifty subjects,

2	make	these	combinations	in	100%	of	the possible cases.
1	"	"	"		93%	"	" "
1	"	"	"		89%	"	" "
1	"	"	"		71%	"	" "
3	"	"	"		60–62%	"	" "
2	"	"	"		53–58%	"	" "
2	"	"	"		41–45%	"	" "
10	"	"	"		33–39%	"	" "
5	"	"	"		23–29%	"	" "
12	"	"	"		10–15%	"	" "
2	"	"	"		6– 9%	"	" "
9	"	"	"		0%	"	" "

Barely one-fifth of these subjects, therefore, are proof against the impulse toward combination, while nearly one-half of them share its influence in one-third of the possible cases. Fully three-tenths of the images, from stimuli expressly chosen to prevent natural combinations, are fairly forced into some sort of relation.

The strength of this mental habit is, in truth, of great significance. The power to relate and to group conscious experiences in complex image or in reasoned conclusion, is the mark of intellectual capacity. The effective thinker possesses not merely discrimination keen enough to reduce to its elements every undistinguished compound of tradition and of immediate experience, but the grasp of mind required to bring together scattered and apparently unrelated elements, that they may fuse into vital and organic compounds.

III. Associations with Childhood Experience.

The conscious life of the child is so rich and so vivid that the images of childhood are, throughout life, most readily suggested. In the first of all experimental investigations of association[1] Francis Galton found a large proportion of childhood associations among a number which he recorded (39 per cent. of 124), and only a small number (15 per cent.) of associations with very recent events. Mr. Galton made, as he says, no absurd profession of being able by these few experiments to lay down statistical constants, and the present investigation was undertaken to discover whether the predilections for childhood association were indeed a universal characteristic, or, on the other hand, either an individual peculiarity or a habit of middle life.

Lists of associated images were obtained from 90 Wellesley students, and from 84 persons in middle life or of advanced age. The suggesting words, expressly chosen so as to invite childhood associations, were the following: "A church, a mother, a silver cup, a dog, a picture, a doll, a doctor, a book, a drum, a slate, a jackknife, a grammar, a school, a father, a bible." The subjects were directed to write the name of the first concrete object suggested by each of these general terms. The results may be compared with Galton's in the following summary:

Records of	Number of Subjects.	Number of Assoc.	Childhood Assoc.	Recent Assoc.
Galton	1	124	39 per cent.	15 per cent.
Wellesley students . . .	90	1127	14.7 " "	32.7 " "
Old and middle-aged . .	87	1226	33.4 " "	30.9 " "

Thus, in spite of the natural connection of the suggesting words with the period of childhood, even the old and middle-aged associate less childhood experiences than Galton did. More important is the fact that, however many the childhood associations, at least one-third of the suggested objects of consciousness, both of young people and of older persons, are connected with the immediate past. The multiplication of child-

[1] 'Human Faculty,' p. 195, *seq.*

hood associations with advancing years is very apparent, but it should be observed that it is not a constant characteristic. Two persons of seventy-three years have each only two associations with childhood, while some of those in early middle life, or even of college age, recur constantly to the earliest years. Evidently the grasp of the recent is heavy upon the conscious life of most of us, and deviations from this type of association are due to individual caprice.

The simple investigations whose results are here reported furnished useful material for the training of first-year students in psychological investigation. The actual experiment was performed in each case by the instructor. Memory and association-lists and the subjects' comments were then assigned to small groups of students for analysis and for tabulation. Finally, all the records were read and all the figures verified by the writer, who takes this opportunity to acknowledge the careful and thoughtful work of the students, too numerous to be named, who have assisted.

MUSIC IMAGERY; A CONFESSION OF EXPERIENCE.

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The power of music to call up particularized images rests upon a process of indirect association which depends for its existence and character upon the individual genius and experience of the hearer. Music is not an intellectual instrument, as language is, having each element connected with a distinct object; and much of the æsthetic enjoyment which it affords is due to this very lack of definiteness in its content. The musical variations of human speech, as Spencer has observed, like the variation in amount and quality of gesturing, are expressive of passion and feeling. They interpret to a being susceptible of like emotion—and often transform in significance—the thought-content of the words which they accompany. The function of music is to indicate or produce a mood rather than to communicate a set of definite concepts.

In speech, each element, the word, and each group of elements, the phrase or sentence, is connected with a distinct image or set of images; it *means* that, and should mean nothing else. And the perfect language is that one which has for each object one symbol and no more, a scientifically complete set of signs, without deficiency and without redundancy.

In music there are no such hard and fast lines of association. The note and the passage do not *mean* anything at all, as the phrase and the word do; and the æsthetic worth of music derives greatly from this plasticity of its symbols, and the wide subjective variation in their interpretation, which allows for the play of imagination and the influence of mood upon them. A piece of music, or certain passages of it, or even its single notes, may *make us think of* any one of a thousand things or experiences, just as the echo of a voice might, or the smell of a flower, or the sight of a glove or book; but the music does not *mean*

these things as the word means the object for which it stands. It means them only because in some way it has become so associated with the past experience that at its occurrence that experience is called up.

The symbols of music might perfectly well be employed, as are the symbols of speech, to express definite particularized images. The variations in symbolism are endless; the sign may be an articulate sound as in speech, a graven or painted mark, as in writing and inscription, a flash of sunlight as in the heliograph, or the manipulation of the fingers, as in the language of the dumb. The intermittent sound of the electrical ticker, the swinging of the brakeman's lantern or the waving of his arms in train shifting, the red cross on the door in plague-stricken cities, the passes and foldings of the fan in a flirtation, these are all symbols having a definite intellectual content; and, though no such relation now exists, each note of the scale and each combination in a chord and composition into a phrase might be made to stand for some definite thought-object, with the result of producing a music-language.

For there are three forms of association which may connect the symbol with the object which it serves to call up. The first is by *convention*, the second by *composition*, and the third by *analogy*. Of the first class, as has already been said, human speech is the type. Its elements are arbitrary signs which by convention we have made to stand for certain objects and ideas, and which by custom we use to express our thought and describe our experience. There is no bond between them, either of likeness or natural connection, other than our will that it should be so. By composition I mean that association between two things which has arisen from their participation as elements in some common experience, by juxtaposition either in space or time. The smell of moist earth brings back a rainy day years ago, spent in tramping among the hills; the string of beads recalls the little playmate who wore them; the sight of a letter which he has forgotten to mail calls up the breakfast table and his wife's charges to the delinquent husband. Frequently in this form of association nothing of the original experience is recalled to mind, but only the mood which accompanied it, and

occasionally this mood remains as an inexplicable whim of association, unsolvable by any assignable reason.

The sampler which violates every principle of good taste may yet be dear in spite of its hideousness, if it once hung upon the walls of a room we loved. And, surely, no redeeming feature can be conceived in the pungent, suffocating coal smoke which is beaten down into this city's streets on a muggy day; yet a gentleman has told me that to him the experience is always accompanied by an overtone of pleasure. He was unable, for a time, to account for this happy mood, but soon traced the cause to a similarity between the smell of Cleveland smoke and that of London, a city which he loved.

This form of association, both in the production of definite images and of a diffused tone of feeling, plays a great part in music. Perhaps in this consists its chief delight, apart from pure sensuous pleasure in the streaming and compounding of melodious sound, that through these fragmentary and evanescent associations it brings before the mind a procession of delightful images, or those still more characteristic moods which thrill and pass with each change in the orchestration. Suggestion by analogy or resemblance is still more widely employed in music to produce the desired effect. The ripple of water over stones, the singing of birds under the open sky, rain, wind, thunder and the ocean surf, the songs of a village festival, the clash and strife of a battle onset, all these are common musical effects, and such descriptive passages depend for success upon their direct imitative character. The images which they invoke are called up through the half-deceptive mimicry of the music, as the face of a friend is called up by the sound of his voice, or the appearance of the fluttering leaves, by the murmur of the breeze which passes through them. The association is simple and direct. Upon this imitative descriptive quality depend our battle pieces, our word music, our thunder-storms, as in Beethoven's *Pastoral* and the overture to Rossini's *Tell*. "All of a sudden the sky grows dark. The woods are silent; shudderings run over the surface of the lake. The water is ruffled. Nature seems to shrink within herself at the approach of the storm. It comes with mighty strides. The

wind sways the tall firs; engulfs itself beneath the towering timber. A first lightning-flash rends the clouds, others follow; then the thunder roars, the hurricane is unchained in fury." Rossini describes all this—and can describe it only—by representative changes in the orchestration, from the subdued murmur of muted strings during the ominous hush of the woods preceding the outburst, to the tumultuous crash of music from all his instruments when the tempest is at its height. At the culmination of this harmonic storm the music stops. A moment of silence follows, in which we seem to feel the hush of exhausted nature, the storm dies away in a muffled rumbling from the drums, and the piece closes with the drowsy drip of water and the murmur of waterfalls swollen by the rain.

All our refined music is characterized by a much subtler and more elusive quality of imitation, in which spiritual passion and endeavor and the many moods of life are represented through a vocal symbolism assisted often by intricate intellectual associations, *motifs* and conventional cues. Take, for example, the overture to Tschaikovsky's '1812' in which the repulse from Moscow is described. The furious *allegro* which forms the main body of the piece is worked up stormily on the strings towards a full orchestration in which the martial notes of the cornets and horns rise high and clear amid the tempest from the strings and wood instruments. Winding in and out of this tumultuous chorus are fragments of the *Marseillaise*. This gradually becomes more complete until, at the moment when it is apparently about to dominate the passage, it breaks and is wholly lost in the thunder of the full orchestra. The victor is sufficiently indicated by the noble Russian hymn with which the overture closes. Hope, fear, tumult, the crash of battle, victory and defeat, together with the names of conqueror and conquered, are set forth here through imitative description eked out by *motifs* and conventional cues, such as Wagner has worked up into a well-defined system of symbols.

But there are other cases of visual imagery accompanying the hearing of music which are not thus easily referable to their sources, cases in which the image is apparently neither suggested by an analogy in the note or passage, nor called up by a

conventional cue, nor assignable to any fact of habitual association or whim of fanciful interpretation. One lover of music has told me that frequently, as she listens to the music of an orchestra, an opening rose appears before her against a dark background. The phenomenon is apparently barren of association: there is the music, an auditory experience, and the rose, a visual experience; they appear together—but why, she cannot tell. The phenomenon is analogous to that of colored hearing and probably to a multitude of other experiences which occur unremarked to every individual. A more connected set of such sensuous images, in my own experiences, accompanied the hearing of one of Schubert's quartettes in the *Sing-Akademie* of Berlin some time ago. To this my long preamble has led and I shall transcribe it as it stands in the notes written the same evening after the performance. As I sat with closed eyes listening to the music a succession of visual images appeared, in almost unbroken series, sometimes unrelated, here and there broken for a moment, but at other times developing continuously into each other for the space of a minute or two, or even more.

The first number, from Mendelssohn's forty-fourth opus, was in progress when I entered, and I remained standing. No visual images accompanied this music. The second was from Beethoven's eighteenth opus, Quartette in A sharp, during the latter part of which visual images began to occur, especially during the andante movement. The third part of the program consisted of Schubert's Quartette in D minor, and it was during the allegro movement, with which the piece commences, that the imagery became a profuse and continuous accompaniment to the music.

For a few moments after the violins began to sound, though I attended as closely and with as much pleasure in the music as subsequently, no images appeared. My enjoyment was in the pure sensation of richly blending notes, and in the thrill of delight at each realized expectation, as note after note fell upon my ear. But in a few moments the music on the stage fell back to a secondary position. I saw suddenly before me, as I sat with closed eyes, a valley with gardens and shade-trees, and a slender rill of water winding down its center. The atmosphere

was warm and hazy and the air was full of light and the humming of insects. And somewhere in the valley a voice was singing, a girl's voice, clear and tremulous. As I looked and listened, a troop of boys and girls tripped down the road. They were dressed in light colors, with flowers and wreaths upon their dresses, and carried tambourines and garlanded poles. As they came they were in continual graceful movement, dancing lightly in time with the music, shaking their instruments, waving their garlands and winding sinuously in and out among one another. Suddenly, as I watched, the valley grew dark and gloomy, the rich, warm hues of dresses and flowers began to fade away, and rain came down in great drops, heavier and heavier. The air was filled with the beating of the rain and the roar of water; the little stream in the trough of the valley swelled into a wide torrent; all the dancers dissolved away but one, a young girl, who remained irresolutely standing, surrounded on every side by the rushing water. Then a satyr appeared, who seized the girl in his arms and went off bounding through the water. After a few yards, as I watched his leaps and the white dress gleaming against the rocks, his hoof slipped upon the wet surface of a stone, and he fell headlong with his burden. At the same moment the whole scene faded from sight. Then I saw a forest and a lane passing through it, with an open space at the farther end, and the sun shining down into it. In the open space stood a tiny schoolhouse, and along the lane came a little girl in a short frock, with feet and shoulders bare; she was tripping as she came, in time with the music below me, and swinging a little satchel by a single strap. Then a group of children—boys and girls—came rushing down the lane like a gust of wind and disappeared as they approached me; then another, and another, and another, in quicker and stormier succession.

This all suddenly vanished and I sat in a theatre before an empty stage. In a moment a clown or dervish, wearing a wide, full gown of brown stuff, ran down before the footlights, danced a moment, and then began spinning on one toe like a teetotum. Then another joined him and another until, I think it was, five stood spinning giddily in a row; and behind them

another line of similarly whirling clowns appeared, until I grew dizzy watching.

All at once, however, I was again out under the blue sky, lying at full length on the bottom of a light canoe which drifted slowly before the breeze. I could see the clouds above me, and feel the rocking of the boat and hear the soft lapping of the waves against its side. Gradually the rocking became slower and more sluggish, as if the water of the lake were turning into treacle which grew thicker and thicker until the motion of the canoe wholly ceased. Then I sat up, and found myself fast frozen into the ice, near by the shore, on which a heavy forest grew. Just opposite me there was a break in the wood where a path led down to the water, and I could see the blue sky through the trees. As I looked, I heard a song, a song from a single voice, across the ringing, frosty air, and a grown boy came round the bend, swinging two water-pails which dangled from the ends of a heavy wooden yoke passing over his shoulders. He came down the bank, stopped at the edge of the water, and struck the corner of his pail sharply against the thin ice.

As the ice broke before the force of the blow this scene similarly vanished and I found myself standing in a great cathedral with long aisles stretching away from me until their farther ends were lost in gloom. Far down the church, or in some side chapel, I heard voices rising and falling in solemn cadences and gradually growing louder and more distinct; then slowly up the aisle filed a double row of monks, in long black gowns with their cowls flung back off their shaven heads. They approached until the foremost stood in full light a few yards from me; when, picking up their gowns with both hands at the sides and holding them wide apart as if dancing a minuet, they began capering noiselessly about. I saw their heavy shoes and rough wool stockings and wondered to see them dance so lightly, when, to my amazement, they rose bodily from the floor, changing form as they went, and passed up through the roof, a flock of swallow-like birds. Again a flock arose, and again, each mounting spirally in a gusty swirl of wings and disappearing, as steam disappears on a frosty day.

Finally, a single bird was left far up in the sky, rocking slightly on poised wings but otherwise seeming to remain motionless.

As I watched it I heard a solitary, long-drawn, flute-like note sounding. And as it sounded I found myself on a seashore with cliffs behind me—I did not see them at all, but knew they were there—and a waste of rocking waves before me. It was night and the sky was dark and stormy. Far away at the horizon line there was a streak of cold yellow light. The air was filled with innumerable spirits all about, in ceaseless, intricate movement, whom I could not see, but whom I felt to be present on every hand. Across the water towards me, as I stood, came a group of voices, like a bit of flying cloud, but vocal, driven before the wind and melted about me, to be succeeded by another and yet another, and, I think, still a fourth.

With the fourth cloud of voices came a sudden break of strong red light in the sky, mounting rapidly from the horizon upward and spreading to right and left till that whole quarter of the heavens was in a glow. With the coming of the light arose a chorus of lusty young voices singing joyously.

Then suddenly it was bright day, and I stood in a road on the mountain side. There were great brown rocks on every side, and low trees here and there, and the sand on the road was red. There was no grass or flower anywhere about, nor any pretty thing, but all the features of the landscape were stern and grand. And close by me in the way was a young man who stood and looked up to the rocks on the mountain side; and once or twice he tried to climb, but could not. And I felt the strong endeavor and longing within him, but did not understand what it sought after, and I felt, too, the presence of an obstacle which could not be surmounted. Then he sat down on a stone by the roadside and laid his head upon his hands, and I felt the load upon his heart and grew hopeless, seeing his despair.

This also passed suddenly away, and I saw a wide old country road running through green fields, with great elms shadowing it; and near by an inn with a heavy wooden table under the tree before it. And down the road came working people, in ones and twos and threes, with rakes and hoes upon their shoulders. They walked slowly and were silent, and

parted as they came near, some going to this house and others to that, the women passing at once in-doors, the men remaining outside putting things to rights, and talking occasionally among themselves. Darkness came on, lights were lit in the houses and the number of men to be seen became smaller and smaller. At last only one, an old man, remained, and as he passed into his house I followed him. It was a bare simple room, containing only a bed and an unpainted table with a single clumsy chair beside it. On the table stood a candle which the old man had just lighted, and a clock ticked high up on the wall. All the other lights had gone out in the village and everything was quiet and silent. The old man went here and there about the room, every moment moving more slowly and feebly, until at last he lay down to rest; once or twice he moved, then there were two long, even, soundless breaths, and all was still. The music had ceased.

The part which visual imagery plays in my psychological world depends greatly upon my physical condition. When thoroughly fresh and vigorous my thinking is slower, more logical, forced and schematic than when I am slightly tired. When actual weariness and languor set in, of course all mental exertion becomes painful and ineffective; but in the earlier stages of fatigue, before this final condition has been approached, a period of cerebral excitement occurs, often accompanied by a slight frontal headache, in which my mental imagery becomes more varied and concrete than the normal. I feel an unusual brilliancy and fertility of suggestion; my mental scenery becomes less schematic and algebraic; comparisons and illustrations suggest themselves on every hand; thought proceeds by object images. At such times my sensation-memory also becomes unusually vivid and copious. There is no longer a dry, hard, logical way of conceiving things, but the sight and sound and smell of the things themselves, which seem to have been stored away in some solitary cell of the mind under lock and key, break forth from confinement and go rioting tumultuously across the stage in a chaos of imagery.

And on this particular occasion these conditions obtained. I had the night before finished a piece of work on which I had

been closely engaged, and all that day was in a state of joyous elation at bringing it to a successful close. The day, too, had been filled with a succession of engagements, which demanded continuous activity of both mind and body, and after taking tea with a friend I had hurried directly to the *academie*, arriving there late, flushed, excited and slightly tired. The phenomenon of visual images accompanying the hearing of music has been, in my own experience, rare; on various occasions such imagery has arisen in my mind, but fragmentary, consisting of single, unrelated forms, which appeared on a dark background, remained visible a moment or two, and then suddenly vanished. And such images as appeared came sparsely during the course of the music, its effect during the intervals being of an altogether different kind. And the occurrence of visual music-images has, among my personal acquaintance also, been rare rather than frequent. The phenomenon is in itself also a curious and interesting one. It is the more interesting to me in that I am usually a relatively poor visualizer. My memory-images are neither copious, nor distinct, nor vivid in coloring. I find it difficult in most cases to recall how any group of things actually appeared; I cannot at all reproduce things in memory so as to be able to run over them again as if they stood once more before me. When I do succeed in gaining in this way a view of any group of things, it has none of the simultaneous wealth and variety of the concrete object-complex; I must recall element by element, and piece the whole together in a series of memory-syntheses. The images which do appear in distinct form before me are almost without exception pale in color and fleeting in character, being rapidly reinstated by successive efforts instead of remaining permanently before me any length of time through their own persistence. Even the faces of my most intimate friends sometimes escape me, and at best are usually remembered only by some individual feature and not at all in the rich objectivity of their actual presence. It is only a flashing remembrance of an eye-glance, or a fleeting smile, or a sudden memory of contour, or gesture, or even a single tone of voice which comes at my call; the sweet human variety of it continually escapes me. But not escapes me

wholly; it is still present as a background and takes up the fragmentary ideas which visual memory affords and suffuses them with feeling, so that I have a very distinct and concentrated apperception of my friend—or whatever the object of memory may be—only it is not habitually a directly visual one. Sometimes I have found it utterly impossible to call up again the face of an acquaintance. No single attitude or expression, no line, or feature, or tone could I lay hold of by the closest endeavor stimulated by intense longing. When my mother died, for three weeks I could neither see her before me, nor remember how she looked, nor recall a single feature of her face. The memory, at the same time, was in other regards intensely vivid and strong. I have a friend of whom this is a typical experience. He has never been able in the slightest degree to recall his mother's face. He *knows* she is dark, with brown eyes, a firm chin, straight nose, face slightly lined, and hair plentifully sprinkled with gray. But he has never been able to put these analytic facts together in any sensational way, so as to *see* how she looks. He cannot even reconstruct his favorite professor's face when his back is turned.

My own enjoyment of suggested visual objects, I mean the form under which things which may be seen enter into my psychological world when remembered or otherwise suggested, is an indirect one. When I hear or read a description of any particular bit of landscape—for example, of standing on the shoulder of a hill and looking out over the valley and its nestling villages, of seeing a tempest in the mountains, or a snowstorm upon the plains, or of sweet, wet meadows lying bright in the slant sun after a sudden dash of rain—what I usually get is not a procession of visual events across the field of imagination, but a succession of emotional attitudes, the feeling which the beholding of the objects themselves would arouse, is called up directly by the description and not mediately through the suggestion of an interposed set of visual images. I get the glow, the thrill, the feeling of exultation, the stormy joy, the still serene pleasure, but not the diaphanous blue haze of the farther hill slopes, nor the ragged masses of thunder cloud torn by the mountain peaks, nor the swirling

white mist of flying snow, nor the waving of wet leaves and shimmering of rain-drops in the sun. This is the most usual form of my experience, but by no means the invariable; visual images are frequently evoked, constantly so, indeed, but the typical way in which such things are represented to me is not through these visual objects, but through the emotional tone which they are suited to call up. But in the experience which I have described, to this syncopated form of suggestion there is added a further stage which involves an inversion of what I suppose is the customary order of suggestion, namely, from intellectual perception through imaginative images to a mode of feeling. For here, apparently, out of the mood directly suggested by the music arose the particularized images which accompanied it. There was no conscious seeking after an interpretation of the music, no endeavor to find a series of objects or actions which, in some more or less fanciful way, would illustrate the character of the music. The scenes arose, the drama was acted with complete spontaneousness; I was as passive as if the figures had appeared before me on a real stage. Indeed they simulated a performance on the boards with great closeness, for the figures had all the vividness of the most distinct memory images, both in detail and coloring. So strong was the impression that, though the memory of the music is so completely gone that I should probably not recognize it again, yet several of the figures and scenes which accompanied it reappear now at the thought of the experience with almost their original intensity, although nearly two years have gone by since then.

I have not tried to trace out any close analogy between the quality of the individual passages and the character of their visual accompaniment. To do so it would be necessary to have a synchronous record of the music and its pictorial content, which would be wholly impossible to obtain. There is, at first blush, apparently a lack of any bond between the two. They occur together, as do the vowel and the color in chromæsthesia, but with the like absence or obscurity of reason. Why, in colored hearing, is one letter accompanied by the sensation of red, another by that of yellow, a third by that of black? The thing

is quite irrational; it can have its explanation only in some chance association peculiar to the individual, or in an original idiosyncrasy of the subject's make-up. And what intelligible connection can be imagined between the character of a piece of music and the slipping of a satyr's hoof upon a wet rock, or the antics of a group of mountebanks, or a school girl's swinging her satchel as she runs home from school? Apparently no more than between the music of which I have spoken previously and its evolving rose. Yet here and there the origin in my own experience of the images which arose is traceable, and in several others a certain connection may be imagined.

The experience is immensely more complex than that of colored hearing, and can scarcely be referred to its type. The intellectual processes whose results are involved are more intricate and highly developed than any which chromæsthesia demands, and fall in with a type which has found many illustrations in my experience. For instance, with the thought of one woman of my acquaintance comes habitually the half-sensational impression of Greek-fire and thunder, with that of another it is of a fountain and the tinkling of bells, of a third it is of a sepia etching. In all these three cases, whatever the mode of origination which established this association, there appears to me a decided fitness in the symbolism, a fitness which, of course, must be felt, and cannot in any exact way be either defined or defended.

In the experience which I have described something of the same nature can be traced. Several of the individual figures I can definitely locate. The singing monks find their original in the pilgrim chorus of Tannhäuser, a favorite scene of the opera which has always impressed me strongly; the lake with its floating canoe is the imaginative revival of an old habit of drifting idly in my canoe on a breezy day without sail or paddles; the troop of children have stepped out of some village festival or other on the stage; and the cloven-footed satyr is some fragment from a forgotten painting. There are several elements, too, which are repeated in the course of the experience. For example, the frolicking youths recur in another guise in the whirling dervishes, and again in the dancing monks; the vanishing rush of children down the lane is one in essence with the

flight and disappearance of the transformed monks, and with the voices which sweep across the water and die away as they approach. And both of these groups of images are strongly suggestive of a rapid passage of music in a rising scale, such as actually occurred several times in the quartette. It is probable that some sort of such vague analogy accompanied the experience throughout, and that out of the feeling directly induced by the music the images arose through a congruity, at least temporarily felt, between mood and figure. The mood was the electrical atmosphere out of which developed the lightning-flash of imagery. The influence of moods upon the character of our associations is at all times very great. As one tone of feeling or another becomes dominant, as in grief or joy, whole groups of objects, or aspects of life, are rendered so eminently congruous or incongruous that they are inevitably either dwelt on or excluded from contemplation. The existence of an intense mood, that is, will so raise the degree of excitability in a whole group of allied images that it requires but the slightest stimulation to touch them into activity. And here, out of the range of images from past experience, whose congruity made them possible subjects of suggestion, those actually arose into consciousness whose greater recency or vividness or superior fitness as symbols, rendered them momentarily the most easily excited.

ON THE EXPERIMENTAL INVESTIGATION OF MEMORY.

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It is the purpose of this sketch to give a critical survey of experimental research on memory. In it I have endeavored to discuss the more important results of this study and to refer to the chief works on the subject.

In regard to this purpose I have been anticipated by Dr. Burnham, who, in his work on the history of the investigation of memory (24),[1] has a most admirable account of the experimental work. But unfortunately this was written in 1889, only four years after the publication of Ebbinghaus' researches. It thus takes into consideration only a very small portion of the work now accomplished. Since then Professor Titchener (75) and M. Binet (8) have written accounts of work done along this line. Each of these is excellent, but, being short, does not cover the whole ground.

It was actual work on memory[2] which caused me to feel the need of a sketch such as this aims to be. Every one who has studied the literature of the subject must be convinced of the general ignorance on the part of the investigator of the work of others. There has been a very large amount of wasted work. The same problems have often been treated several times without the slightest advance being made. The literature on the subject of experiments on memory is far larger than any would imagine, and the problems of this sphere are of great significance. It thus seems desirable to give some account of

[1] The numbers in parentheses refer to the numbers in the bibliography at the end of the article.

[2] I hope later to publish certain investigations of my own on memory for pressure and memory of sound intensity.

the literature and to state in a general way the nature of the problems.

The Methods.

In the experimental investigation of memory the general methods of experimental psychology are, of course, employed, the method of right and wrong cases, so called, being most in use. With these general methods we have nothing to do, for memory research has brought about no modification of them. However, leaving these aside, there are several methods peculiar to the sphere under consideration which have been developed and which are of the greatest importance.

In every experiment on memory there are of necessity two terms—*A*, a stimulus of some sort which constitutes the thing to be remembered, and *B*, an expression on the part of the reagent of the state of the condition of the memory *A*. Take, for example, an experiment on the memory for squares. It is first necessary to show the subject a certain square (*A*), then after the lapse of a definite interval of time to require him in some way to express the state or condition of his memory for that square (*B*). Now the methods of the experimental investigation of memory are to be distinguished from each other solely by the character of the *B* of the experiment; that is to say, they differ from each other as the nature of the expression differs. This is proved by the fact that in his research on the memory for squares Professor Baldwin (1) in each experiment gave the subject a single square to remember (that is to say, the *A* term remained constant), although he made use of all of the known methods.

The first distinction between the means of artificially expressing the condition of a memory, which is thus the distinction between memory methods, rests upon a difference between two general phenomena of memory, namely, those of reproduction and recognition. It is a well-known fact that former experiences are much more easily recognized than reproduced, and that we all, *e. g.*, understand many more words than we are able to use. As far as experimental methods are concerned, in reproduction we call up an old experience, unaided directly

by any immediate expression from the outer world; while in recognition we simply feel the identity of a term already in consciousness with one which was there in the past. In the experiment itself this difference takes the following form: In the method of reproduction the subject, having been given some stimulus, is called upon after the lapse of a certain time to reproduce that stimulus without any further aid from the experimenter. In the experiments on memory for squares he is required to draw the square on paper; in those on verbal memory he is required to write or pronounce the words which he was told to remember. On the other hand, in the method of recognition, the subject, having been given the stimulus to be remembered (*A*), is again given one or more stimuli and is required to pronounce upon the relation of identity which the latter holds (or hold) to the former. Thus the method of reproduction is to be distinguished from that of recognition by the fact that in the former only one artificially given stimulus is required in each experiment, while in the latter at least two are necessary.

Now, while the method of reproduction cannot be further reduced, the method of recognition breaks up into two minor forms. These, in the terms of Professor Baldwin, are the methods of identification and selection.[1]

In the former of these the second artificially given stimulation consists of one term only. The subject is required to compare a single present stimulus with a past one of the same general sort, in order to determine whether they are identical or not; hence the name, method of identification.

In the latter the second stimulation consists of more than one term. The subject is given a series of stimuli and is required to select from it a single term which was formerly given alone.

In order to illustrate these three methods we may make use of the case already mentioned in which each was employed by Baldwin and his colleagues (Warren and Shaw) in their investigation of the memory for squares (1, 82).

1. Method of Reproduction.

A normal square of 170 mm. was shown and then immediately removed; after the lapse of a given time (20, 40 min., etc.) the subject was required to draw the same on paper.

[1] For the origin of these terms see below.

2. Method of Identification.

A normal square was first shown, as above, and afterwards another one either of the same size as the first or greater or less. The subject was asked to determine the quantitative relation of the second to the first, *i. e.*, whether it was equal to, larger or smaller than the first.

3. Method of Selection.

A certain time after the removal of the normal a variety of squares of different sizes, one among which was equal to the normal, was shown and the subject told to designate one which seemed to be of the same size as the normal.

These three methods are thus seen to be distinguished by the second term in the experiment, or by the means of expression. In the first method it is not dependent on an artificially given stimulus. In the second method it involves one such stimulus and in the third it involves several. Therefore, these methods seem to be exhaustive. As a matter of fact, all past experiments on memory have been done in accordance with one or more of them.

The distinguishing and naming of the methods of recognition and reproduction were due to Wolfe (85, p. 537), who thereby endeavored to describe the difference between the method first employed by him in his investigation of tone memory and that used by Ebbinghaus. It seemed easier to Wolfe to get at the nature of memory through the phenomenon of recognition than through that of reproduction. For this reason he objected to the method of reproduction used for the first time by Ebbinghaus and developed his method of recognition. This particular form of the method of recognition was afterwards called the method of identification.

The first formulation of the methods of identification and selection is due to Professor Baldwin, and appeared in a report to the American Psychological Association in December, 1893 (80), which was followed by a fuller description published in May, 1895 (1).

In 1894, in his 'Psychologie Experimentale' (10, p. 76), Binet described the three methods and characterized them as

those of reproduction, comparison and recognition.[1] This was done in apparent ignorance of the work of Wolfe and Baldwin. Inasmuch as these investigators have anticipated him, their terms should evidently be retained.[2]

Both Baldwin and Binet seemed to consider the method of identification to be of the greatest worth. The method of reproduction is adapted to the study of verbal memory. Indeed, it alone can be used in this work. No attempt has yet been made to investigate the memory of words by any other means. But in other spheres its value is greatly impaired by the fact that the motor factor involved in the act of reproducing inevitably introduces an element into the reproduction which is not an expression of the memory image and which cannot be determined. Moreover, the reproduction is always conditioned by the ability of expression possessed by the particular individual. For instance, in the case of squares the individual *A* may have a more accurate memory for geometrical figures than *B*, but may have had less practice in drawing them, in which case *B* might be accredited with greater accuracy than *A*. It is just for this reason that the method of reproduction is applicable to the study of word memory, for in this case all ordinary individuals have had sufficient practice in expression to rule out the effect of different degrees of ability in regard to expression as possessed by different subjects.

The method of selection should be avoided when possible, for the reason that when a series of stimuli of the same sort is given the attention is normally drawn to the middle of the series. The law of contrast always affects the apparent character of each member of the series. It was found, for example, that in the work on memory for squares done according to this method the apparent size of each square in the series from which the normal was to be selected was conditioned by its relation to the other squares. Moreover, it is only when the stimuli are spacial that they can be given simultaneously.

[1] Binet elsewhere (9, page 608) substitutes the term selection for that of recognition.

[2] See in this connection foot-note to page 608, No. 9 of the bibliography, and to page 175, Vol. V., of the PSYCHOLOGICAL REVIEW.

The method of identification is by all means the simplest and most satisfactory of all memory methods. The only objection to which it is open is the practical one that it must always be coupled with the general method of right and wrong cases, which requires a very large number of experiments in order that there be a sufficient ground for a valid induction.

The Material.

By the material of memory researches I mean the stimuli used in the experiments.

There are in general two ways in which a thing may be remembered. In the first place it may be reproduced so that it appears in memory relatively the same as it appeared in reality. Or, in the second place, it may be remembered by means of concepts. On the one hand we recollect the thing immediately; on the other mediately through some *tertium quid*. In mediate memory, if the term may be used, what we remember is not the thing itself, but our classification of it; that is, we remember certain concepts as related to the thing. If, for instance, we endeavor to reproduce on paper the peculiar curvature of a line we had once seen, it will be necessary to bring the line up to the 'mind's eye' as it appeared in reality. But if we desire only to recollect its length it will be sufficient to remember that we judged it to be a certain number of inches long.

Now, the material used in experiments on memory is sharply divisible into two classes, according to the way in which it is remembered. If what we are to remember is some delicate shading of color or some fine variation of pitch, it must be recollected immediately; but if it be simply some concrete object in its general character or some well-known complex of sensation it must be recollected mediately. For the former objects there are no terms in language, but the latter are instantly classified and named. Now, the material to be recollected immediately is relatively simple, while that to be recollected mediately is complex. This is amply shown by the list at the end of this section. It may thus be well to retain the terms simple and complex to signify a necessary and important distinction between the sorts of memory material.

The material chosen by Ebbinghaus for his experiments consisted of about 2,300 nonsense syllables (33, p. 30). Those were preferred to words on account of their relative simplicity. But even meaningless syllables are complex when compared to simple sense impressions, and Ebbinghaus remarks that something still more simple would be desirable, for in the learning of a nonsense syllable there are involved the senses of sight and hearing, together with the muscular sense in respect to the hand and the vocal organs. Wolfe criticises Ebbinghaus still further in this respect, and chooses for his own researches the simplest material of which he can think, namely, the delicate changes of pitch.

Oddly enough, this criticism which Wolfe raised against Ebbinghaus has not retained its force. The material used by the latter is still largely employed. Both of these researches have served as prototypes for all that has since been done. Both Ebbinghaus and Wolfe start out with the purpose of investigating the general laws of memory. Although Ebbinghaus expressly mentions the character of the material to be remembered as a condition of recollection, he and Wolfe both tend to treat memory as a function which operates according to formal laws independent of content to be remembered.

If this presupposition were valid the material best adapted to this line of the work would no doubt be that which is as simple as possible. But later researches show that the presupposition is not true. The accuracy of recollection is largely conditioned by the character of the content to be remembered. Thus, while the memory for words, pitch, space, etc., falls off rapidly in respect to accuracy as the time interval increases, memory for time itself, as far as it has been investigated, shows almost no diminution of accuracy as the time interval increases (Cf. 58). It is, moreover, probable that the memory for certain contents exhibits peculiar phenomena not to be noticed elsewhere. There is no positive reason for believing that Wolfe's formula for expressing the relation of time to the accuracy of memory is necessarily true for any content other than used in his experiments. From the standpoint of the experimenter there are as many different memories to be investigated as there are peculiar contents to be

remembered. The general problem of memory, as it is now conceived, is that of tracing the transformations which take place in each content as it passes through time. The researches of Ebbinghaus are still significant as throwing light on conceptual memory, and Wolfe can claim no more than a corresponding value for his experiments. Thus the question of the relative worth of the different materials, raised by Ebbinghaus and Wolfe, is seen to be without significance.

In all experiments on memory it is necessary that the subject display a certain amount of inaccuracy. This inaccuracy is obtained by making the material of the experiment difficult to remember. Now, two means have been employed to secure this, and they differ according to the nature of the material. Where the content to be remembered is complex, that is, where it is mediately remembered, it is rendered difficult of retention by making it large. Thus Ebbinghaus usually employed about fifteen nonsense syllables as the material for a single experiment. All of these he could, of course, not hold in mind after but one reading.

In the case of experiments on memory for a simple content, that is, where the object is recollected immediately, it is not only necessary to make the material difficult of retention, but it is also necessary to exclude any possible classification of it, for there is in general no choice as to the way in which a thing is remembered. If possible, it is grasped and held conceptionally. Moreover, on account of the extreme fineness of language and the high ability in the formation of new class ideas on the part of the subject, it is not an easy matter to exclude the classification of the material and thus mediate memory. But this exclusion is accomplished and the desired difficulty of retention obtained by using the just discernible shadings of sensation as the material for these experiments. For the threshold values of sense impressions are, at least at first, incapable of being classified and also extremely difficult of retention.

The following is a list of some of the materials which have been used in experiments on memory.

Simple.

Intensity of Light (49).
Intensity of Sound (48).
Pitch of Sound (85).
Localization through Touch (51).
Space Relations through Touch (76).
Pressure (76).
Muscular Sense (Weight) (76).
Active Movements (55).
Odor (48).
Geometrical Figures through Vision (1).
Time (58).

Complex.

Sentences (13).
Words (15).
Syllables (33).
Letters (27).
Numbers (30).
Concrete Objects (46).

The Problems.

In order that a certain object be remembered it is necessary, first, that it be experienced, and, second, that some image of it be retained after it has gone. Furthermore, the conditions which govern the chances that a certain object be remembered depend, first, upon the depth and clearness of the impression which that object made on me in my experience of it, and, second, upon the transformation which my image undergoes in the temporal flow. In general we may say that my memory of a thing depends upon the way in which it was received into consciousness and the way in which it is retained. Other things being equal, that object will be best remembered which made the deepest impression on me—which was most thoroughly perceived or apperceived; other things being equal, that object will be best remembered whose image undergoes the least transformation in time. Thus, to the experimenter there are in general but two great memory problems. The first of these is that of dis-

covering the relation of the character of the reception of an object into consciousness to the accuracy of recollection; the second is that of ascertaining the relation of the transformation of the memory image to the same.

I. The reception of the object into consciousness.

There has never been any attempt made to determine directly the depth of the impression which a certain object under certain conditions makes on consciousness. The reason of this lies in the fact that it is impossible to measure directly such a factor. However, it has always been known that the accuracy of our memory of a thing depends upon this term. Moreover, there have been many attempts to measure it. But in these attempts the unit of measurement, instead of being directly that of the depth of the impression or the thoroughness of its association, is the transformation of the memory image in time. The presupposition which underlies all of the experiments on the conditions of the reception of the object as affecting the accuracy of recollection is that there are but two general conditions of the accuracy of recollection. These, as just stated, are the character of the reception of the object and the transformation of its image in time. Now, if this be true and if we can perform an experiment in which the only variable term is that of the conditions governing the reception of the object, we shall be able to measure the depth of that reception in terms of the transformation. Suppose, for instance, that we desire to experiment on the effect of the repetition of an experience on the depth of the impression which that experience makes on us, and thus on the accuracy of our recollection of it, we may arrange our experiments as follows: Let our subject be given an object, say a series of nonsense syllables. Let him repeat them twice, and after ten minutes require him to reproduce them. Then let him repeat an equivalent series five times, and after the lapse of an equivalent period again require him to reproduce. The time interval and other conditions being constant while the number of repetitions is variable, the relative number of errors in the reproduction will serve as adequate terms for the measurement of the relation of the depth of the impression made on the subject by two repetitions to that made by five. That is, we measure the depth of the

impression made by a certain experience in terms of the transformation of the image thereof, for evidently the errors made by the subject in his attempt to reproduce the experience are due to changes in his memory image of that experience. That is, other things being equal, the memory image of an object which has made but a slight impression will be sooner dissolved than that of an image which has made a deep impression.

Now, in the text-books on psychology there are put down at least two factors governing the reception of an object into consciousness which condition the accuracy of recollection. These are attention and repetition. But according to the results of the experimental investigation of this subject these do not complete the list. For it has been shown experimentally that in the case of repetition the rhythm governing the repetition also conditions the probability of the experience repeated being remembered. Moreover, the general character of the object to be remembered has been shown to condition the depth of the impression made by it, and thus also of its chances of being remembered.

(*a*) Attention and repetition.[1]

Common experience teaches and the text-books state that these two factors condition the depth of thoroughness of the impression which an object makes on us, and thus condition the memory of it. Experimental research on this subject has done little more than confirm what was already known, which, however, is not to say that it is, therefore, of no value. Data of considerable future worth have been collected, and from them we have already attained a somewhat more definite conception of the enormous rôle which these two phenomena play in our memory. Indeed, it would be almost possible to state the matter mathematically were that of any immediate psychological significance.

(*b*) Rhythm and the general character of the object.

These two factors are not usually put down in the text-books as conditions of memory. Indeed, it is one of the peculiar achievements of the experimental work on this subject to have

[1] See the researches of Daniels (30), Smith (72) and Bourdon (21) for attention, and those of Ebbinghaus (33), Smith (71) and Hawkens (39) for repetition.

discovered the former and to have developed the nature and importance of the latter.

It was Ebbinghaus who first brought out the effect of rhythm on memory. Afterwards Müller and Schumann (53) discovered that in repeating a series of nonsense syllables there was an almost irresistible tendency to break the series up rhythmically into sections, and that this so far conditioned memory that syllables belonging to the same metrical whole tended to be held together.[1] Thus rhythm was found to condition the reception of the object into consciousness, but in regard rather to its apperception than to its perception. Its effect has been tested only in respect to memory for complex contents where the material is easily associated. If possible, it would be an extremely interesting problem to examine the relation of rhythm to memory for simple contents.

Bigham and Münsterberg (6, 56), Hawkens (39), Kirkpatrick (46), Smith (70), Cohn (27), Whitehead (84), Tschisch (76) and others have examined the effect of the character of the object to be remembered on the accuracy of our recollection of it.[2] The particular results of these researches are too varied and often too contradictory to be stated here. Their especial value is largely pedagogical, the chief point of interest being the discovery of the great importance of the muscular sensations (*i. e.*, motor terms) in our memories. This, again, is one of the places in which experimental psychology is justified of its task.

In general we may say that the reception which an object gets into consciousness depends very largely upon the sort of object it is and upon the sense by which it is perceived.

The study of the conditions which govern the depth and thoroughness of the reception which an object gets into con-

[1] See also Cohn (26, 27).

[2] By the term general character of the object I do not merely refer to differences of character which are objectively recognizable, such as the difference between a strain of music and a nonsense syllable, but also to differences which are due to the sense by means of which the object is received into consciousness. On the ground of this I would speak of the logically same nonsense syllable as forming four different possible psychological objects according to whether it is read, heard, spoken or written.

sciousness needs overhauling. Much is lacking from the standpoint of the experimenter and very much more from the standpoint of the analyst. An example of this is the lack of distinction between the attention and the character of the object as conditions of memory. An attempt should be made to analyze these conditions into their lowest terms.

Almost all of the work on this subject up to the present has been done on complex material. It seems to me that it would be very profitable to extend the former experiments so as to cover the problems of memory for simple material. For instance, as far as I know, there has never been an attempt made to determine the effect of repetition on immediate memory. This is also true of rhythm and attention.

II. The transformation of the memory image in time.

The laws which govern the retention of an experience are evidently different from those which govern its reception. An object which has once been fully experienced, but is now for the time forgotten, will come up again in the future, bearing the marks of the unconscious processes which determine its transformation while in retention. Now, experimental psychology has succeeded in doing little but describe in part these marks. The processes themselves have not been investigated—the conditions of the transformations have not been experimented upon. However, something has been done in the way of determining the nature of the transformations themselves.

If we omit from consideration the relations among ideas, we may say that the memory image of an object in consciousness may differ from that object itself in three ways: It may be more or less distinct, it may be greater or smaller, or it may be of another quality. The forces working upon the memory of a thing to dissolve it may affect it as to distinctness, quantity or quality.

The method of determining the transformation which the image undergoes in time is just the opposite of that of determining the effect of the conditions of the reception of the object. In the latter case the forces operating upon the image in time were kept constant, while the conditions of the reception of the object were varied. Now the conditions of the reception must

be kept constant, while the time interval (*i. e.*, the time between the moment of the experience itself and the moment of recollection) must be varied. That is, in order to discover the changes undergone by the memory impression we must always start out with exactly the same experience to be remembered.

It must, of course, be understood that the forces which cause our memory of a thing to become inaccurate are unconscious, and that the object on which they operate is always unconscious. The memory image is the conscious factor which corresponds to the latter term.

(*a*) The fading of the image in time.

The growing indistinctness of the memory image in time is the most evident of these three expressions of the forces which may attack the memory impression, and the only one which has attained any degree of recognition. In the handbooks it is generally spoken of as the effect of time on memory. The description of the stages in the fading of the memory images of different objects has been the theme of most of the experimental work on memory, and almost all of the treatises put down in the bibliography contain something on the subject, while many of them are devoted entirely to it.

As far as is now known, our memory of all objects is subject to fading. To this, however, there is one surprising exception. Paneth (58) found that for time intervals not exceeding five minutes, which were the only ones examined by him, the memory image for time intervals fades so little, if it fades at all, that it was impossible to determine by means of the methods he employed that it does fade. In all other cases, as far as is now known, the growth of indistinctness in the image sets in immediately. Indeed, there is evidence for the belief that the memory for several sorts of material is more accurate at an interval of two or three seconds than at any interval of one.[1] Perhaps, as Lewy suggests, this is due only to the necessary haste in judgment on the part of the subjects experimented upon. At any rate, the discovery is an important one and should be held in mind in respect to experiments on other subjects, es-

[1] See Wolfe (85), Lewy (51) and Bigham (6).

pecially in respect to researches which involve the comparison of non-spatial stimuli.[1]

If we leave this phenomenon aside as one which perhaps is to be accounted for on other grounds than those which properly govern the transformation of the image itself, we should naturally expect, from all we know introspectively concerning the course of our memories of the different objects, that the rate of fading of the memory image is a mathematical function of the time interval. Such in general it has been found to be.[2] However, Wolfe (85), Baldwin (1), Bigham (6), Lewy (51) and others have found certain wide variations in the memory curve in its relation to time, which suggest a decided periodicity in the memory for certain contents. Thus Wolfe, who was the first to investigate such disturbances, discovered that in all the subjects examined by him there is a point in the temporal curve lying between fifteen and twenty-five seconds at which the memory for pitch is more accurate than at a point five seconds before.

There are in some cases forces at work on the memory image which cause decided aberrations from what would naturally be supposed would be the curve for the fading of the same. What these forces are no one has yet discovered, and the problem seems to be a difficult one. In this connection it must be remembered—and the point is one which almost without exception has been overlooked—that if there be such a thing as quantitative or qualitative change in the memory image such change would affect the accuracy of memory. The matter will be referred to again, but I may here suggest that, as far as I know, there has never been an attempt made to discover the relations of such possible transformations in the image to the general

[1] In most of the experiments attempting to discover the relation of Weber's law to sound the two sounds to be compared follow each other at an interval of a second or less.

[2] The formula of Ebbinghaus expressing the relation of the accuracy of memory to time is as follows:

$$\frac{\text{that which is remembered}}{\text{that which is forgotten}} = \frac{k}{(\log t)^c}$$ and that of Wolfe:

$$\frac{\text{that which is remembered}}{\text{that which is forgotten}} = \frac{k}{\log t} + c.$$ In both of these formulæ k and c are constants.

curve for the accuracy of memory. Until the possible effects of such changes are abstracted from this curve, we may never be sure that the curve for the increasing inaccuracy of memory is the curve for the fading of the image.

(*b*) The quantitative change in the memory image.

By the term quantitative change I mean those changes in our memory of a thing which cause the image of the object remembered to seem greater or less in respect to duration, extent or intensity than the object itself seemed to be. That is, the transformation of the image would be such that if an object equal to the object remembered be presented to consciousness it will be judged unequal to it.

Tschisch (76), Lehmann (48), Merkel (Philos. Studien IV., 137), and Starke (Philos. Studien, III., 270) have discovered that when two sounds objectively equal in intensity are given, one after the other, the second is judged to be the louder.

Leuba, in his admirable experiments on the classification of artificial stars of different intensities (50), has shown that in case a normal light stimulus of a certain intensity is held in memory the comparative stimulus, if its intensity be small, is judged to be greater than it actually is, and if its intensity be great is judged to be less than it actually is.[1]

Baldwin and his co-workers (1, 82) found that squares of certain sizes held in memory are judged to be greater than the objects of which they are the memory images.

Moreover, I may be pardoned if I state that in experiments on the memory for pressure performed in the Princeton laboratory last winter I found that if two successive pressures of the same objective intensity be given one after the other the second will be judged to be the less.[2]

[1] The effects of Weber's law are, of course, eliminated.

[2] These experiments, the results of which will, I hope, be published later, extend only to cases of about 25 gm. at intervals of about three seconds.

In this connection see also T. Bolton (18, page 304).

This account of the work on this subject would be larger were it not for the fact that, for reasons stated above, researches made according to the method of reproduction are without significance in this connection. Thus the investigations of Binet and Henri (14) and Jastrow (45), which otherwise might point to a quantitative change in the memory image, have to be ruled out, because the method used prohibits any such deduction.

In the case of the intensity of sound we find a decrease in the intensity of the memory image; in the case of light either a decrease or increase in the intensity of the image according to the intensity of the object itself, and in the case of squares and of pressure a quantitative increase in the image.

Most of the work here represented consists of researches on small or unknown intervals, but it is amply sufficient to prove the existence of this remarkable phenomenon of the quantitative change in the memory image.

As yet there exists no entirely satisfactory treatise on this subject. Indeed, as far as I know, there is not a single experimenter on memory who has quoted or used the results of his colaborers in this particular line. In order that the researches on the quantitative change of the memory for any particular content be adequate, it would be necessary to determine first the relation of such transformation to the quantitative aspect of the object remembered and to the time interval. No one treatise has yet examined both of these problems.

In respect to the relation of the quantitative change in the image to the quantitative aspect of the object to be remembered we have the most interesting work of Leuba, and in respect to the relation of the same to the time interval the researches of Lehmann and Baldwin and his colleagues. The result of Leuba's investigations has already been stated. Lehmann, in his work on sound, found that the quantitative change noticed in the memory image increases with the time interval. Baldwin and his colleagues in their work on squares came to the same conclusion. However, their results are not altogether convincing.

There have been three attempted explanations of this phenomenon.

The first of these is offered by Starke, Lehmann and Tschisch. These men noticed the diminution in the intensity of the memory of sounds. This they explain by the assumption that the memory of an experience is in general less intense than the experience itself. This broad explanation seems to be rendered worthless by the experiments of Leuba and myself, in which an increase in the intensity of the memory image as compared to the original experience was noted.

The second explanation is offered by Professor Warren (82), who attempts to relate the phenomenon of the increase of the image in memory for squares to the operations of Weber's Law. In my opinion this theory violates the facts themselves.

Finally Leuba offers the hypothesis that there is formed in consciousness a sort of unitary typical representative for each class of experiences, which is a residuum left over from all the perceptions we have had. Toward this middle and typical term our memory images are drawn. Thus our memories of very intense perceptions tend to became less intense, and *vice versa*.

This, it seems to me, is the most rational of all the explanations and is certainly borne out by the facts of Leuba's own experiments. But whether it will hold in regard to the memory of other contents than the one investigated by him I do not know. The problem is certainly one of great importance and interest.

(*c*) The qualitative change of the image.

As far as I know, there is not a single work on this subject. I myself have gone over Wolfe's very complete tables and have found in them data which enabled me to draw the curves of qualitative change. This curve for each experimentee exhibited certain variations from the curve for the fading of the image, but I was unable to make the particular curves agree or to obtain any satisfactory results.

If there be such a thing as the quantitative or qualitative change of the memory image in time, such changes will affect the accuracy of recollection just as much as the fading of the image. The presupposition of the handbooks and of practically all of the experimental investigation of memory is that the curve of the increase of the inaccuracy of recollection in time corresponds to the curve of the fading of the memory image. What the experimental investigation of memory most needs at this juncture is thoroughgoing work on the transformation in time of the memory image of some simple content, which would show the curve of the fading of the image, that of the quantitative change and that of the qualitative change, and find relations of these curves to each other. Perhaps were such a work consummated some new light would be thrown on the phenomenon of the periodic changes in the memory image.

III. Certain conditions which affect both the reception and retention of the object to be remembered.

Up to this point we have treated of the reception of the object into consciousness and the transformation of the image of the same in time. There are several individual factors which condition both and which have been investigated experimentally. These are the age,[1] sex,[2] race[3] and health[4] of the individual who receives and retains.

The work on these subjects has been of considerable value and many details of psychological and pedagogical significance have resulted from it. It, however, has developed no startling results except perhaps the one which seems to show that girls have better memories than boys and colored men better than white.

As was to be expected, the general accuracy of memory in school children increases with age.

The work on the effect of race and pathologic conditions on memory has been extremely meagre.

It has been my purpose in this paper to describe the chief problems belonging peculiarly to the experimental study of memory and to examine the methods by which that study is to be pursued. I have thus carefully omitted from consideration all work on cognate subjects, such as association of the mental span.

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DISCUSSION AND REPORTS.

PSYCHOLOGY AND EDUCATION.

In the last number of the PSYCHOLOGICAL REVIEW Professor Cattell discusses an article which I published in the *Atlantic Monthly*, and comes to the conclusion that the theses of my paper are in contradiction both with his convictions and with my own earlier writings. I cannot agree that there is any difference at all between my present and my earlier attitude, but, above all, I am very glad, indeed, to find that, at bottom, even Professor Cattell does not really disagree with me.

Professor Cattell considers as the three essential points of my paper, first, an unfavorable criticism of Dr. Scripture's recent book, 'The New Psychology;' secondly, the epistemological claim that mental processes cannot be measured, and, thirdly, the practical suggestion that teachers ought not to expect any help for their methods of teaching from the study of experimental psychology.

I do not discuss the first point, which is, in any case, unimportant, as I deny that a criticism of the book referred to was intended. That is sufficiently indicated by the fact that Dr. Scripture's name was not mentioned at all in my whole paper. Professor Cattell is quite right that if I wish to criticise a book it is not enough to criticise only one single point in it. The fact that I gave no name and mentioned one objectionable feature only, shows, I trust, sufficiently that I cared only for the criticism of a special theory, not of the volume which proposed that theory, and I had thus, it seems to me, the full right to spare my readers all the uninteresting reasons why I consider that book a bad one. But the second and third points alone are of scientific interest.

I have maintained, and stand to it, that psychical facts are as such not measurable because they cannot be understood as multiples of constant units. Professor Cattell says that in so maintaining I ignore my own previous work, as I have "published numerous measurements made by a psychologist in a psychological laboratory for the analysis of psychological processes." Certainly, I have done just that, and I hope to go on, but is there the slightest contradiction? To make measurements for the purpose of the psychological analysis is not identical with the measurement of psychical facts. That many of our

laboratory measurements done in the interest of psychology are measurements of physical objects, for instance, air waves or ether waves or space distances or pressures, is a matter of course for everybody; all that I claim is that this is the case also with all the other measurements in the interest of psychology, for instance, with those referring to the intensity of sensations or to the duration of reactions. In calling such time-measurements or intensity-measurements physical, their importance for the purposes of the psychophysical laboratory is, of course, not touched at all.

But do we really disagree here? Professor Cattell writes: "Whether these should be called physical or psychological measurements is to a certain extent a matter of nomenclature," and he finally adds: "The epistemological questions involved are important, but are such as psychology as a natural science can ignore." We agree here perfectly. The question involved is indeed not a psychological one, but I have nowhere said that it is. The limits of psychology are not the limits of the scientific world; my paper was a philosophical one and worked almost exclusively with epistemological tools. To be sure, for psychology it may be quite indifferent whether you call these measurements such or such, the only thing important for the progress of psychology is that they are made and well made, but that by no means relieves philosophy of its duty of examining without prejudice the ultimate nature of the facts and of calling the psychical facts immeasurable, the more so if important philosophical problems of other kinds are connected with that decision. On the other hand, as psychology in its special investigations has nothing to do with the problem, there is no reason why we should hinder psychology, in its practical use of an abbreviated terminology, calling a physical measurement made in the interest of the qualitative analysis of psychical facts shortly a psychical measurement, and the time-measurement of those physical processes which are the physiological accompaniments of psychical processes simply psychometric studies. Professor Cattell is quite right: I have done so myself, and I do not regret it. The psychologist philosophizes only in holiday mood, and if he uses on working days his convenient unphilosophical terminology he should be free to indulge himself. Of course if we misuse the careless abbreviations of a convenient terminology as a basis of a classification or even of a whole psychological system, the result must be complete confusion, for in the basis of the system and in the principles of a classification psychology must be penetrated by epistemological thought.

Still further reaches, it seems to me, the agreement of Professor

Cattell and myself in the last and practically most essential point, where at a first glance the antagonism appears to be still deeper: the relation of psychology and education. I said: "This rush of the teachers toward experimental psychology is an absurdity." This Professor Cattell regards as contradictory to my earlier views. He says that I took, some years ago, a 'more correct attitude,' as I introduced a paper of mine with the words "The experimental study of memory is important both for psychology and for pedagogics." But are these two statements really in any way opposed to each other? In my opinion they are exactly complementary. Just because I hope that a systematic scientific pedagogics will make in steady coöperation with educational specialists full use of the results of psychology, including those of an experimental character, I fight against the dilettante ways in which individual teachers manufacture their private pedagogics from odds and ends of psychological experiments.

I do not wish to go here into details, as the September number of the *Educational Review* contains, under the title 'Psychology and Education,' a long article in which I treat these and the related questions of child study in all their ramifications. But I am confident that such detail is unnecessary to bring to light the harmony between Professor Cattell and myself as soon as this distinction between the individual teacher and the science of pedagogics is emphasized. Professor Cattell says that psychology has the same importance for the teacher as physiology for the physician. But did any one ever really become a good physician by the study of physiology? Certainly not. A man might spend his whole life in a physiological laboratory and yet be unable to make the slightest use of all his frog and dog experiments in his medical practice, if he had not learned scientific medicine. His laboratory knowledge would only confuse him; he would be absolutely lost if he tried to transform privately his physiological knowledge into therapeutical prescriptions. A special science of medicine must do this work, which is by no means the business of the individual practising physician. In the same way the teacher has not to hope that he, by his individual effort, will work out any useful pedagogical prescription if he peeps into the experimental records of our psychological laboratories; he must rely on pedagogy as a special science to link psychology with the art of teaching. Tact and sympathy lead a teacher who has no pedagogical training safer than such dilettante psychology which must confuse and irritate him, and I cannot believe that my warning against such home-made pseudo-pedagogy is not in sympathy with Professor Cattell's own beliefs. I stick, therefore, to

my previous conviction: "This rush of the teachers toward experimental psychology is an absurdity."

HUGO MÜNSTERBERG.

THE NEW CASES OF TOTAL COLOR BLINDNESS.

The results of the interesting observations upon a color-blind person (and in part upon two other color-blind persons) which were made by Professors Hess and Hering, and which are reported upon on another page (p. 532), are summed up by the authors in these words: "The hypothesis of König and v. Kries, in accordance with which the totally color-blind differ from those who have normal vision either by the absence or by the functionlessness of the cones, finds no support in our observations." This conclusion is expressed with the carefulness and freedom from exaggeration which are customary with its authors; it is true that the case of achromasia here reported upon does nothing to strengthen the theory referred to. (I shall speak of the case in the singular, as do the authors in their conclusion—as that of Frl. F.—though many of the observations made upon her were confirmed in the other two instances.) If normal cones are wanting in the fovea, then it would seem that the situation must be capable of being described in one of the three following ways: (1) there must be no visual elements there which are capable of performing any function; (2) there must be cones which (whatever their external shape may be) are undistinguishable from rods as regards their effective photochemical contents, or (3) the regular rods must extend over the whole surface of the retina, including the fovea where they are normally wanting. In the case of Frl. F. it seems to have been conclusively made out that there was no defect of vision in the fovea, and hence the first state of things above described is distinctly excluded. The case is not so simple for the other two alternatives; experimental evidence was obtained which had some bearing upon the third alternative, but not conclusive bearing; if there had been normal rods in the fovea, and if all normal rods contained the visual purple, then it would seem (if we adopt the hypothesis that the growth of the visual purple is the source of the adaptation to a faint light) that Frl. F. ought to have been deficient in that defect from which the normal eye suffers, the night blindness of the fovea; that is to say, her fovea ought to have undergone, unlike the normal fovea, the same adaptation to a faint light as does the rest of the retina. This was found not to be the case, and it can, therefore, safely be inferred that her fovea is,

as in the eyes of the rest of us, deficient in *visual purple;* but it cannot be inferred with equal safety that it is not provided with *rods.* It is well known that not all the rods contain the visual purple; we are expressly told by Kühne that that pigment is wanting not only in the extreme periphery, but also in the rods which are found infrequently in the immediate vicinity of the fovea. If, therefore, we had had to make a prediction as to whether, on the hypothesis that there were in a given instance rods instead of cones in the fovea, those rods ought to furnish ground for the existence of the Purkinje phenomenon, it would have been distinctly the safer course to suppose that they would not do so; and certainly it cannot be inferred from the absence of the Purkinje phenomenon in the fovea of Frl. F. that her fovea is wanting in rods, but only that it is wanting in the pigment of the rods. It would be rather a far cry from the one inference to the other even if all other rods, as far as known, did contain the rod-pigment; but since the rods which are in the immediate vicinity of the fovea are known *not* to contain it, the inference is a quite impossible one. It seems odd that this obvious objection to their argument did not suggest itself at once to these distinguished experimenters.

The second hypothesis of those above enumerated is not affected by the case of Frl. F. If the fovea is supposed to be occupied by cones which are simply in a sufficiently undeveloped state not to furnish any sensation of color, no reason exists for saying that their development has been arrested at one stage rather than at another, and certainly no reason exists for saying that they must of necessity contain the purple coloring matter of the rods. And, in fact, Hess and Hering do not affirm that their experiments have force against the hypothesis regarding the nature of total color-blindness in this one of its three forms (which is the one in which I defend it).

But, even if it had been rigidly shown that the achromasia of Frl. F. was not in any way due to the rod-like structure of the visual elements of her fovea, the hypothesis which is here in question would be very far from being disproved by her case, or even by all three of the cases which were apparently of a very similar nature. It is true that three cases are a goodly number when it is considered how rare well-authenticated cases of total color-blindness are, but it is not sufficient to upset the conclusions which are to be drawn from a much larger number of cases which are different from these. The additional phenomena which are usually attendant upon achromasia—nystagmus, avoidance of a bright light, and diminished visual acuity—were *all* wanting in the cases here tested; these were, therefore,

plainly exceptional cases and not such as should furnish a basis for general conclusions. Professor Müller, among others, has pointed out that nothing can be farther from a legitimate deduction from the present view in regard to the distinct function of rods and cones (which he also adopts) than to suppose that *all* cases of congenital dyschromasie are due to anomalies in the retina. When the dyschromasie is acquired, it is well known that it is most frequently an attendant symptom of disease of the optic nerve or of the higher centres. It is markedly different in this respect from night-blindness, which is the characteristic symptom of pigmentary degeneration of the retina, which latter disease is now supposed to start from a pathological proliferation of cells in the pigment epithelium, a membrane which, since it secretes the rod-pigment, may be regarded as of the nature of a gland, and hence subject to diseases of its own. (Since the night-vision, which night-blindness is the absence of, is achromatic vision, or gray-vision, an additional strong ground is here to be seen for the assumption that the latter is the function of the rods exclusively.) But if acquired lack of the color-sense is seldom, if ever, associated with defects in the retina, it would be strange if congenital color-blindness were not at least sometimes due to defects of the nervous apparatus. The result of this work of Hess and Hering, therefore, need not be anything more than to force an upholder of this hypothesis to the assumption that there are two forms of total color-blindness—one (with nystagmus, foveal blindness and avoidance of high lights) due to a defective condition of the cones, and the other, without those attendant symptoms, due to some difficulty in the nerves of conduction or in their receiving stations. And this latter form is much the least frequent. It is certain that the totally color-blind boy whom Professor König had in his laboratory in the summer of 1894 had no vision in his fovea (though no one had ever discovered it until after the knowledge of the normal night-blindness of the fovea caused it to be specially looked for). Of a group of small, bright objects, he constantly lost one out of sight; and it was only necessary to stand in front of him, and ask him to look at you, to see plainly that in his effort to look you straight in the eye he was obliged to turn his eyeball markedly to one side.

C. Ladd Franklin.

THE CRITERIA OF MENTAL ABNORMALITY.

It is my purpose to discuss briefly the criteria which theoretically discriminate normal mentality from that which is abnormal. Psy-

chiatry is a well-defined portion of the science of psychology, but the line which divides the subject-matter of the one from that of the other is almost wholly a line of practice and not of theory. I, therefore, intend here to offer a few considerations on the subject of a purely speculative nature, and shall have little or nothing to do with the technical symptoms of insanity, however important these may be from the therapeutic and systematic points of view.

To a chance visitor upon humanity, say from the planet Mars (supposing him of a general and very natural curiosity), the conduct of the patients in a hospital for the insane would seem to differ in no respect, touching the matter in hand, from that of the more numerous persons in the same city whom he would meet. He would distinguish differences in the actions of the two classes compared, to be sure, but the one would appear to him as natural or as strange (whichever you please) as the other. The notion of mental abnormality would not suggest itself to him, strange to the long-descended customs of the race of civilized Earthians. In fine, mental health and derangement are wholly arbitrary abstractions, dependent on inherited or consistent habit or expediency, or at most are the unessential, and so to say uncertain, forms into which the conditions of our world have at this time forced development. The attendant in the hospital most likely is firmly convinced that this man who has been for years his charge, boasting of divinity or raging and in deepest grief, by turns, is wholly of another mental standing from himself, is hopelessly and mortally 'mad,' a god and a beast by turns. Yet the difference in these two men from a broad and theoretic view-point is no widely yawning chasm of essential contrariety, but the, as it were, chance matter, perhaps, of a few ounces of blood per day in two equal brains, or the separation of neurons measurable in thousandths of a millimeter, or of changes as yet wholly, as far as the bodily correlates go, beyond our guess. Yet the one is properly said to be sane and the other crazy, as any one of our own social mind could quickly see. But one of the two is a citizen who fully earns his wage and living at a disagreeable trade; the other a menace to the life and morals of society. Taking a less conspicuous example, a case that the specialist would class as hysteria or as 'degeneration,' the problem seems less easy of solution, but it is the same. On some topics this man is peculiar, has strange notions we say, soars too high in his fancy, grovels too deep in his vices, fears things many of his fellows would scorn to fear, or jumps, 'Miranda'-like from some Belvedere, with hope too strong. It is such cases that suggest inevitably how disease merges into health. These are the cases about which the judges and the doctors hold long parleys, wondering who is sane.

For our purposes here, from an epistemological point of view, mind, whether called right or wrong, is one—the necessary correlate of more or less unknown processes going on at once in every portion of organic masses of matter called living bodies. Here is a constant relation between parallel series of events, and from one aspect, namely the purely theoretical, abnormality of mind would indicate only a departure from this parallelism between body and soul. This is on the metaphysical presumptions that what is is fully real, and that the ethical problem is something independent of the biological. Yet I would not urge this criterion save as a logical and indeed artificial mark of difference.

What, then, is it, we may properly ask, that Science means by the term 'abnormal mind'? The discrimination is on the whole a vital one to humanity, and yet the State year by year finds it more difficult, as also more important, to decide which of men and animals should be placed beyond the opportunity of doing harm. The relation between crime and insanity yearly becomes better understood, but both of these are closely involved with health in a way that makes the discrimination difficult. Justice demands a close and ever closer definition of society's dangerous members, for the protection of both the others and themselves. This is one of the problems of the social psychology of the future.

To the judge in trials for suspected crimes responsibility is the all-important question for determination, and in practice this is not seldom quite impossible. The criterion here is an ethical one: Does the accused know the difference between right and wrong? Has he, as a part of his mind, that 'moral sense' upon which Shaftesbury laid so much stress two hundred years ago, and if he has it in the average degree, does there exist in his nature that proportion between conscience and desire, arising respectively from soul and body, necessary for resistance to the temptations which bodily or biologic needs force upon it? In short, does he know right from wrong and is he of average strong mind? The practical difficulties are here immense. The psychology, however, is quite simple, the presence of one intuitive sense, simple though inexplicable, being in question, and its strength relative to the love of pleasure, which is the stronger. So the whole problem of responsibility seems to reduce to the relative intensities of two senses, the hedonic and the moral. This criterion applies to such cases of doubtful abnormality of mind as lead, or are apt to lead, to fractures of the social justice and of the rights of men.

Another criterion of deranged mind is to be found in a departure

from the *average* mentality of general humanity. It is on this basis that mental condition is judged by untechnical people, by the world at large, and it forms the unwritten rule of nervous specialists as well in an ill-defined yet important way; diagnosis is regularly made almost at sight in the 'run' of cases. The healthy average man does certain things in certain ways; the alienated mind, with other or even with like life-purpose, does similar things in other ways, and we term him mad. But it is only from our one arbitrary view-point that one can say properly that the actions of the deranged are less in worth than those of the average man. The melancholiac as well has purposes: to escape poisoning, roasting, or what not; the paranoiac has plans often astonishingly ingenious, often as strongly and as clearly defined as those of the members of the board in lunacy who yearly visit him; who shall pretend to say that to the Absolute his plans are less real, less 'normal,' than theirs? The hysteric has intentions, and if by chance her purposes all tend to what we call an undue exaltation of the biologic ego, who will assure us positively that the biologic aspect, of her self is less real than our selves who are classed as sane? It is probable that in an exaltation of the personality lies the essence of most if not all insanity, and the metaphysical values of the self are not open to easy understanding nor are they to be judged off-hand. Man, from the very duality of his mortal nature, is a prejudiced judge of other men's minds. If from the average we gauge the abnormality of mind we must remember constantly that this average is no absolute thing, but that it varies constantly from year to year and in different lands even of Earth. The Eskimo thinks the Parisian deranged, and could even a seer of ancient Nineveh spend a week around lower Broadway who will say what he would *not* think!

To determine the average which is the basis of this criterion of abnormality is, of course, thus, socially, impossible, but to fix average modes of conduct and action given certain conditions, despite the complexity, is not without the sphere of science when by the very greatly needed development of individual psychology sufficient understanding shall be reached. How do different persons do the same act, and how are they to be classified accordingly? This is a definite problem for psychology and quite in the line of these speculations.

In attempting to define the difference between normal and abnormal mind we have come as a final criterion to something at least theoretically more satisfactory than either of the preceding considerations. It emphasizes anew the both vulgar and classic opposition between the common and the egotistic good, between altruistic benevolence and

selfishness. As an individual and with only individual responsibilities and duties the deranged man's conduct may be as satisfactory and complete as that of one classed empirically as sane. His nutrition may be as good, his form as sleek and comely, his strength normal, and his body to all appearances, perhaps even microscopic, wholly sound. Even the mental aspect of his organism may possess all the requisites of proper function—good sleep, clear ideas, memory unimpaired, imagination very likely better than the average, sensations and feelings normal, and, be it emphasized, conscience wholly clear. With all of these and what they imply, with soul and body in evidently proper order, a man or woman may yet be the most dangerous of lunatics, the maddest of the mad. But turning to the vast *social consciousness* of which this willing subject is part, inquiring in what degree of harmony this person's life-purposes stand therewith, and the discrepancy at once is seen, his lack, the nature of his inward dissonance, the reason for his life none too much confined. Humanity's consciousness, too, has purposes and plans, and they proceed inevitably to their grand fulfillment. It is because the purposes of this suspected person (and whether free agent or automaton matters not here) run counter to this evolutionary current, that unavoidably he is overwhelmed and forced beneath and drowned. Neither anatomical nor physiological nor psychological nor yet personal, in a sense, is the deranged subject's defect, but it is *sociological* and against the evolving purpose of the race. Any given case may, of course, be defective in each of these respects, as most are in some of them, but the defect essential to abnormality certainly finds its place relating far outside the individual in the complex intention of the race, be that intention moral or unmoral, racial or cosmologic, ill or good. This is the one unrelative standard by which all vital subjects may be judged. To be insane is to be out of tune, not with the laws of psychology or of physiology, nor yet of the State, but with a broader and more essential tendency—the purpose of the world, it may be of the universe.

To this conclusion not metaphysics alone, but also empirical science points. *Reason* is the aspect of mind which men in general have deemed the highest and most dignified power of soul, be it called by whatever particular name. It is that endowment by which certain species, or certainly one species, of the animal kingdom understands the most real nature of things and their relations—the *meaning* of the ceaseless Life of change in which all our experience is passed. Reason is the just comprehension of cause and effect, or common sense. Now

only a part of the accepted varieties of insanity imply disturbance of this, the crowning power of mind. Mania, for example, is only an unusual hurrying of the psycho-physical action of the higher animals, involving as essential no disturbance other than one of a temporal sort. Melancholia is, on the other hand, the reverse of this, a slowing of the life, a too long continuance of painful thoughts. Dementia paralytica is more of a bodily disease than the two just mentioned, and epilepsy, dementia, idiocy, even more. It is in paranoia that we see a loss of reason in the technical sense of the word, for it is a disease characterized by a confusion of the relations of cause and effect quite as much as by the systematization of delusions based thereon. Here is a state of mind wherein the meanings and purposes of things are deranged, a general disturbance obtaining in the egotism of the subject leading him into greatly wrong relations with himself, with things, and with society at large. This is the typical instance of mental abnormality where is seen at once perverted psychic action and a basal disagreement with the racial Plan.

GEORGE V. DEARBORN.

THE PLACE OF EXPERIMENTAL PSYCHOLOGY IN THE UNDERGRADUATE COURSE.

All of our first-class universities have now psychological laboratories, and the same is fast coming to be the case with those colleges that give any considerable attention to graduate work. Should the colleges that confine themselves to undergraduate work also aim at establishing such laboratories? An article in the PSYCHOLOGICAL REVIEW (July, 1895, Vol. II., page 382), 'The New Psychology in Undergraduate Work,' answers this question in the affirmative, and urges that just as laboratories for chemistry, physics, etc., were at first opposed, but are now regarded as indispensable to every college, so the psychological laboratory ought to be brought in as soon as possible to a place beside the others, and experimental psychology made a part of the undergraduate course. The few remarks which I have to make on the negative side of this question are intended to elicit discussion, and in no wise to disparage the value of experimental psychology in its proper place, which I believe to be in the university or graduate school, and not in the college or undergraduate course.

While the university aims at the increase of knowledge, the college aims at the diffusion of knowledge; while it is the aim of the university to train specialists, it is the aim of the college to educate

men. It is this difference of aim that makes, to my mind, the place of a new subject of investigation like experimental psychology the university rather than the college. The question for the college in determining its curriculum is not "How I can best advance this, that or the other science?" but "How can I best educate these students intrusted to my care for a short four years; how shall I go to work to give them the most thorough intellectual discipline and the broadest culture?" It is the student rather than the subject that the college ought constantly to keep in view. The question then to be settled in determining whether experimental psychology shall be admitted to the undergraduate course is not how will this affect the advance of the science? but is it the best material for the education of the student?

Let us consider this question with respect to the interests of two classes of students, the general and the special. By special students in this connection I mean those who intend to make psychology their specific life study; by the general those who have no such special interest in psychology. As far as the general student is concerned I believe that such time as can be devoted during the college course to scientific work, whether for the purpose of scientific training or that general knowledge of nature such as should be demanded of every college graduate, would better be spent in the study of the sciences already well developed. The time at his disposal can be employed better on such subjects as physics, chemistry, biology, astromomy, etc., whose fundamental principles are already ascertained and whose general pedagogical procedure is already well established. Such time as he has for the study of mental phenomena will hardly exceed what is necessary for a good course in general psychology. If it be urged that experimental psychology should take the place of general psychology, I should reply that the general psychology is a necessary prerequisite to any real comprehension of the aims of experimental psychology. It is necessary for one to have at least an outline of the whole mental field and some knowledge of the problems of philosophical psychology before he can realize the true value of the minute and painstaking investigation of those mental phenomena that are open to experimentation. Now, if it is granted there is no place for experimental psychology in the course of the general student, is it not demanded for those few students who may be intending to specialize in psychology later?

The college, to be sure, while making the intellectual discipline and the general culture of its students its first aim, may not improperly endeavor to provide among its electives courses introductory to the specialization to be pursued in the graduate and professional schools.

But experimental psychology is a science of the cross-roads; it occupies the ground where physics, physiology and general psychology meet. The best foundation, therefore, which a student can lay in his college days for original work in this subject later is to acquire as broad and thorough a knowledge of and training in physics, physiology and general psychology as possible. Thus we conclude that from the point of view of both the general and the special student there is no call for experimental psychology in the college, but that its proper place is in the graduate school, and that the true interests of the science will be furthered by recognizing this.

These views, I fear, will appear quite heretical to many progressive psychologists. No one, however, will welcome criticism of the position taken more heartily than the writer. This is a practical question for many of our colleges at the present time and one well worthy of careful discussion.

F. C. FRENCH.

VASSAR COLLEGE.

PSYCHOLOGICAL LITERATURE.

Ethics. By WILHELM WUNDT. Translated by EDWARD BRADFORD TITCHENER, JULIA HENRIETTA GULLIVER and MARGARET FLOY WASHBURN. Volume 1, Introduction: The Facts of the Moral Life. Volume 2, Ethical Systems.

Since its first appearance, in 1886, Wundt's Ethics has had an eminence in Germany accorded to few other books on its subject. More professorial and remote from the nation's life than Paulsen's, of less value to the special student than Simmel's and several other discussions of not half its own range, Wundt's treatise still gives in an exceptional degree the impression of a large man dealing with a large subject For this reason, and in spite of the extremely German style of the original, this graceful English rendering seems not inappropriate. Books of this sort, books which are the adequate expression of a full and weighty personality, are more native to England than to Germany. Wundt will be made at home among us, and in the stylish garb given him by these two skillful ladies will not appear a foreigner. Yet widely as the book is likely to be read, both on account of its intrinsic merits and because of the great service its author is known to have rendered in a neighboring field, it will hardly be admitted by English readers to that small group of epoch-making books produced in a single quarter century by Mill, Spencer, Sidgwick, Bradley, Green and Martineau. With these it will justly be compared; but its fundamental doctrine will be found to be less exigent and less intimately related to practice than that of some of these books, the number of moral facts observed to be less extensive than in others. Wundt does not offer such spiritual sustenance, nor any such body of minute and scholarly matter, as our half dozen greatest ethical writers present. His reputation in psychology might lead one to expect a compact mass of weighty and criticised material, difficult for any one not a specialist to comprehend. It is important, therefore, to say that nothing of the sort is here. These are two volumes of entertaining reading, in which the general course of civilization is broadly and suggestively traced in a way which is likely to offer more difficulties to the technical student than to the novice. The flowing sentences of the translators, accordingly, fit the scheme better than Wundt's own formal

and elaborated style. Great praise is due both to translators and editor for turning German so tangled into English so simple, and thus bringing into prominence the really untechnical character of the book.

The plan of the original included four Parts : First, a summary of the moral facts as they appear in life; second, a sketch of the opinions which have been held in regard to them; third, a determination of the laws or principles involved in them; fourth, an exhibit of the forms which society assumes under their guidance. Only two of the four Parts have as yet been translated. The reviewer of these has, therefore, the ungracious task of discussing a fragment, a fragment from which fundamental doctrines are intentionally excluded. "Throughout these opening sections," says the author (p. 325), "we have applied the terms good and bad, moral and immoral, in the sense which attaches to them in the not very definite language of everyday life. It would have been foreign to our immediate purpose to raise the question whether these terms really attain to sufficient clearness in the general consciousness, or whether they can be precisely defined at all."

Since, then, the plan of the work postpones to the third part all ultimate questions, nothing is left for the reviewer to examine except that plan itself. A start implies a method, and a method may well enough be considered before its results appear. I shall confine my review to a single question connected with the first Part, the Part entitled 'Facts of the Moral Life.' I wish to set forth more precisely than the author has seen fit to do the reasons why this portion of the work has been given its present position. These reasons are not at once apparent to the hasty reader, and the purpose of the volume may in consequence easily be misconceived.

Ever since Bacon's time investigators have been warned to make their facts clear and sure before attempting to theorize. But they have remained obstinately lax about doing so. Ethical writers, in particular, mix explanations of ultimate principles with records of events in a truly bewildering fashion. Wundt's seeming offer to pen up the facts required by his investigation in a first part, and then to devote a later part to the analysis of them, is accordingly grateful. But consideration suggests difficulties in the plan. How can we know what facts to choose out of the teeming world? We must not seize events of all sorts and merely because they have happened accept them as material for our study. We need not facts simply, but moral facts; and the selection of these involves the employment of standards, which we must first possess. He who will examine the

facts about oak trees must already be able to distinguish oaks from maples, and both from rocks, winds and people. Some acquaintance with oak structure is a condition precedent to gathering oak facts. It is true this acquaintance may be but superficial, while that final acquaintance which results from comparative study of selected specimens may be solid and profound. All I wish here to call attention to is the popular delusion—which Wundt might seem to be sanctioning—that in a scientific investigation it is well first to assemble the facts and subsequently to analyze them. Evidently nothing of the kind is possible.

And these difficulties, which attend all attempts at sundering the interlocked processes of fact-determination and reflective comparison, appear in the ethical field with an additional complication. Granted that the moral facts could be independently gathered, they would have little value for ethics. Wundt devotes a considerable section of his introduction to establishing a contrast between the normative sciences, of which he counts ethics the basal member, and the descriptive. In the latter we are concerned with what has happened; in the former with the worth of what has happened. That John has lied is as genuine a fact as that James has told the truth, and the two have equal interest for a descriptive science like psychology. But neither could become a subject of ethical interest until, possessing some standard of what is ultimately desirable, we should find that the one of them accords with this standard better than the other. Ethics is a systematic assessment of the relative values of human conduct. Its chief problem is, accordingly, the establishment of a standard by which these values may be tested. No accumulation of facts can erect such a standard. Judgment of values and description of facts are disparate processes, and can never grow from a common root. Ethics, strictly speaking, is not concerned with facts.

These are commonplaces. The distinction between the descriptive and normative sciences is familiar to all students of ethics. Familiar, too, are the attacks upon it made by the neo-Hegelians. Wundt accepts, or rather, champions it; and it is, therefore, plainly impossible for him to do as his title would suggest and make this Part a description of facts, in order subsequently, in his third Part, to seek their norms. Normative sciences, he has already told us, are not thus begotten of descriptive. It is true that the *hysteron proteron* which such a method involves is partially covered by the developmental mode of collecting the facts. Wundt does not draw them fully formed from the individual consciousness of to-day, but he looks for them as they

are progressively disclosed in the history of the race. He begins his study at a time when the moral facts are tinged with a minimum of ethical character, and he follows them up from stage to stage, showing with great beauty how they have attained their present color. The straight road to ethics runs not through metaphysics or psychology, he tells us, but through anthropology (p. vi). Accordingly this first Part has four chapters; in the first the ancient moral records of language are examined, in the second those of religion, in the third those of social custom, and in the fourth the conditions everywhere attending the growth of morality. These chapters are full of interest, full of acumen. The many steps through which barbaric man has climbed to moral civilization are vividly depicted as an orderly, almost a logical, sequence. And if some hypothetical history is employed to fill the gaps of authentic record, the writer does not take his history too seriously nor fail to warn us how dependent on philosophic imagination such anthropological surveys must be. "Savage ideas are often very difficult to ascertain with any degree of accuracy, while the tracing of their development is more difficult still. Dependent as they are on oral tradition, with all its chances of loss and confusion, they are probably not seldom incoherent in themselves, while they must appear still more so to the outside observer, who has to guess at possible inner motives hidden under an exterior of conduct that often seems wholly incomprehensible" (p. 64).

Why, then, is such a natural history of ethics undertaken? What purpose is this first Part intended to serve? If simple observation can give nothing normative, and if the facts of descriptive anthropology are so uncertain, why postpone the search after norms for an inquiry how man has come to his present estate? To this important question Wundt gives no very clear answer. The sections of pp. 12–18 are intended to make the matter plain, but they leave the reader still in uncertainty. In my judgment the reasons can best be found, and the plan of the work most satisfactorily understood, by a study of Wundt's subsequently announced (p. 330) 'Law of the Heterogony of Ends.' There being (p. 206) "no morality save as mediated by motives that are originally of a non-moral character," primitive man must have often sought ends which had in his eyes little or no moral quality. But (p. 238) "practical life outran theoretic construction." The results of action were moral, and gradually instructed him who performed it. "The end attained (p. 292) ennobled the motives that prompted to its attainment." For (p. 330) "owing to the constant influence of accessory factors, the result of every act of choice is as a

whole not congruent with the end ideated in the motive. But those elements of the result that lie outside of the original motive are eminently fitted to become new motives, or elements in new motives, from which new ends or variations of the original end arise." Thus, through reflection on what has been performed, "the result of action (p. 260) is gradually transformed to object of action, replacing the original motive." Indeed (p. 247), "the most important law of human development is the law of the unlimited formation of new motives from given effects."

To the illustration of this law the whole Part is devoted. Before exhibiting the principles of morality in their abstract form, Wundt has wished to trace how the mind of man has become progressively aware of them in connection with its daily doings. Not that ethical events can be explained by history, nor even by evolutional history. Wundt is under no illusion here. He sees that the earlier stages of morality must be explained by the later, not the later by the earlier. "Aristotle (p. 247) remarks—and in so doing shows himself to be far in advance of theories of society set forth in later times—that, although the state is, as a matter of historical development, the last term in a series of human associations, it is, nevertheless, implicit in the relation of man to his natural environment, and therefore logically prior to the individual." But evolutional history, Wundt believes, furnishes to morality all its subject-matter, all that on which reflection is to operate in bringing before a continually clearer consciousness moral elements which in the original motive were unperceived. The most elementary motives, consequently those in which ethical apprehension least appears, are the desires for the common means of life—food, shelter and clothing; then follows religion, a factor to which Wundt attaches extreme consequence; and, lastly, the love of beauty; the whole continually guarded by custom, whether of word or deed. Through these agencies results are produced which have an unintended moral worth. The effects of morality thus come into existence long before moral intentions appeal to consciousness. By them consciousness is steadily instructed, and through reflection a new and more moralized species of action becomes possible. This higher conduct itself again passes through similar stages, and a door is thus opened to endless progress. To permit us to watch this process, and to see how the moral mind of man has been awakened through its own performances, is what Wundt attempts in his first Part.

I will not criticise this interesting theory. To make a great book more intelligible by clearly formulating its basal principle has been

my aim. Nor need I spend time over the second Part, a history of ethics from Socrates to Spencer, put into 150 pages. This section of the work cannot be said to have any large value. In their preface the translators recognize the inutility of it so far as concerns the history of English ethics, but hope it may usefully supplement Sidgwick in its view of the Continental schools. My own impression is that it will be found as little helpful for the one purpose as for the other. It was unfortunate that the demands of a systematic plan, or even—as Wundt himself suggests (p. vii)—incidents in the growth of his own thought, should have induced the author to interpolate into an important book a section of such cramped and inferior merit.

G. H. PALMER.

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Outlines of Descriptive Psychology. A Text-book of Mental Science for Colleges and Normal Schools. By GEORGE TRUMBULL LADD, Professor of Philosophy in Yale University. New York, Charles Scribner's Sons. 1898. Pp. 428.

So considerable a portion of psychological publications appears in the form of text-books that these deserve a wider as well as more careful scrutiny than text-books in better-established disciplines usually receive. While text-books rarely enjoy a considerable longevity, they frequently live vigorously for a moderate life-time and bring to the successful author an influence which could hardly come in any other way. In our own country, where the pedagogical direction of the student's efforts is more closely superintended than elsewhere, the text-book naturally reaches a high degree of development both in numbers and quality, so that a vigorous competition accompanied by the usual eliminative processes attending the survival of the fittest is constantly going on. The last two decades particularly have brought about a great variety of text-books of all shades and grades, and almost every American professor of psychology of wide reputation has made or is planning to make a contribution that shall be available in the class-room. Among these the volumes prepared by Professor Ladd occupy a high place alike in popular esteem and in the judgment of specialists. He was the first to present in English any considerable account of the modern researches which may be grouped about the terms physiological or experimental psychology, and this he has done in a larger and a briefer volume. That other and in a measure more extended field of psychological inquiry, in which measurement and precise correlation with bodily processes are largely excluded, Pro-

fessor Ladd has surveyed in a volume entitled 'Psychology, Descriptive and Explanatory.' The present volume may be looked upon as an adaptation of the preceding for the use of less mature scholars. It is not an abbreviation nor a condensation of the more extended work; but in tone and treatment, as well as in the selection and perspective of topics, it forms a distinctive and useful volume.

The most distinctive feature of the volume is the prominence of the genetic phases of mental processes; for of the two parts into which the volume is divided—'the processes of mental life' and 'the development of mental life'—the second is much the larger. But the term development is liberally—perhaps even loosely—interpreted; tor under this head is found not a detailed account of the unfoldment of the senses and movements, or of the more complex processes, with the progress of years; but rather a general analysis and explanation of sensation and perception, of memory and imagination, of inference and judgment, of emotions and will, with frequent reference to the underlying principle that these functions fluctuate and grow, vary and form types. A list of chapter headings of the volume would furnish a very indefinite clue to the contents and the specific character of the treatment. That is suggested, however, by the term descriptive, the fundamental conception being that the psychologist's first and foremost task is to describe systematically and accurately mental processes and conditions, and further so to relate and combine these descriptions that an explanation or an illumination is the achieved or contemplated result. To this must be added the conception that states of consciousness are considered to be the prime content of psychology, and that introspection is the indispensable method of studying consciousness. This field and this point of view seem most congenial to the author; and the result is a sustained interest and a lightness of touch, the absence of which students have often noted with regret in Professor Ladd's other works. In its adaptation to the student's needs it may be pronounced the most successful of the author's text-books.

JOSEPH JASTROW.

A Primer of Psychology. By E. B. TITCHENER. New York, The Macmillan Company. 1898. Pp. xvi+314.

In the preface to this little volume Professor Titchener states, with admirable clearness, the purpose of his work and its relation to his well-known *Outline of Psychology*. The *Primer* is planned for use as a text-book in high schools and normal schools. It is "conceived from the same general standpoint" as the *Outline*, but in it "the ex-

position is simpler and the range wider." The author has "followed current usage in treating of perception and idea as different processes and in restricting the application of the phrase 'association of ideas;'" he has "added chapters on Abnormal Psychology and on the Province and the Relations of Psychology;" he has "throughout laid greater emphasis upon the fact of mental evolution;" and has taken advantage of recent investigations "to improve certain of the sections that deal with Sensation and Perception." But "on points of systematic psychology—in method of order and presentment, in advocacy of the experimental control of introspection, in ascription of the highest value to analysis—the *Primer* and the *Outline* are, naturally, in entire agreement."

As the psychological public is familiar with Professor Titchener's positions and has discussed alike their merits and their defects, the chief question of interest concerning the present book is a pedagogical one, its adaptation to the needs of teachers and students in upper schools and its value as a first text-book in the science. In this respect it can hardly be said that the result is altogether successful. The difficulties in the way of success, of course, were very great, greater in psychology than in almost any other of the sciences. And opinions will differ concerning the merit of any endeavor to surmount them. But, in the judgment of the reviewer, the *Primer* could have been improved if the plan of the treatise had been somewhat changed and if somewhat different methods had been adopted in executing it. Thus, the additional matter, absent from the *Outline*, is of questionable value in the *Primer*. The author is indisputably correct in contending (pp. 286–288) that the chief business of an introductory work of this kind is to discuss "one special set of psychological problems: the problems of normal, adult, human, individual psychology." Why, therefore, devote so much of his scanty space to problems which lie outside these limits? For instance, out of a total of three hundred and four rather small pages, abnormal psychology and the chapter on the province and relations of psychology absorb thirty-eight, while two such important topics as sensation and perception receive but forty-seven pages between them. Again, the book lacks a clear statement of the classification of mental processes which it adopts and the consequent order of treatment. The psychologist, of course, will understand these without an express outline, and there are hints here and there (*e. g.*, pp. 22, 187–188) which will guide the attentive reader of moderate psychological acquirements; but in a primer for high schools and normal schools few things are more de-

sirable from the start than explicit analyses of the subject-matter and of the plan of the discussion. Once more, Professor Titchener has embarassed his discussion by his choice of a point of departure. It was a matter of careful consideration with him, we are told, whether to introduce his readers to psychology "by the way of a general account of scientific method or by the way of brain anatomy and brain physiology" (p. vi). In the event he decided for the former alternative, compressing his sketch of the cerebral organs within the narrowest limits (pp. 13–14). In the opinion of the reviewer the general position is correct; but the necessity of so abbreviating the account of brain and nervous system, even in view of the choice made, is not evident. Finally, there are certain difficulties introduced or enhanced by the limitations of an elementary text-book. The positive, not to say dogmatic, treatment of controverted questions noted by critics of the *Outline* is very marked in the *Primer* (the emphasis laid upon mental evolution adding problems of biology, etc., to the unsettled questions of psychology proper). The necessity for brevity and simplicity sometimes lead to vagueness. The dilemma between general statements of only relative exactness and the limitation of principles to the point of weakness—which confronts every teacher as well as every writer of elementary psychology—is not always escaped; although, if a choice must be made, Professor Titchener's adoption of the former alternative is clearly preferable to the use of formulæ so guarded that their meaning becomes a vanishing quantity.

Over against these subjects of criticism, the reader or teacher will find in Professor Titchener's little work elements of great pedagogical value. The style is simple and flowing; there is an abundance of illustrations, chosen often from the best literary sources; there are many chapters and sections (*e.g.*, the account of emotion, the discussion of action, etc.), which, given the author's standpoint, are done with great skill; the experimental suggestions scattered through the text, or, in particular, grouped in the questions and exercises at the ends of the chapters, will greatly aid in the development of experimental work. The brief list of apparatus and materials at the close of the book will be a boon to many teachers who seek to avail themselves of laboratory methods in the pursuit of the science but lack the practical knowledge of how to begin. Thus, in spite of defects, the treatise will receive a hearty welcome and may be expected to achieve a wide success.

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Logic, Deductive and Inductive. By CARVETH READ, M.A. 8vo, pp. 323. London, Grant Richards. 1898.

The pedigree of Mr. Read's book is avowedly from the works of Mill, Bain, Venn and Keynes. The 'introduction,' however, shows a conception of the subject that is more practical in its bearing upon general discourse than is common with English treatises on logic. Jevons probably gave this tendency its impulse, and Bain, with his interest in educational problems, either shares the honor of starting this new impulse or followed in its wake. The acknowledgment of Bain's influence by the author explains his own tendency to recognize the practical side of logic as the reason for its existence and place in an educational curriculum. Even Keynes has felt the necessity of making some concessions to the criterion of utility and gives some space to practical examples. Mill, without adapting his work to class-room needs, has all his natural sympathies in the same direction. Hence we can understand why Mr. Read defines logic "as the science that explains what conditions must be fulfilled in order that a proposition may be proved, if it admits of proof." The idea that it is the science of the formal laws of thought has dropped into the background. Much of the author's discussion of the subject shows a decided interest and consistency in this view of his science, and the work is in many respects one of the best of its kind. It is not so elementary as Jevons' nor so complete as Keynes'. It takes a middle ground, and treats of induction as Keynes does not. Throughout the work the illustrations and general method of treatment are preëminently designed to effect a practical service for the student. There is also a great deal of discussion that can only remotely affect practical matters. Such, for instance, is the chapter on causation. This may have a speculative value for logic, but it is hard to see how any philosophical discussion of that idea can condition the principles of ordinary practical discourse. Mr. Read is evidently struggling between two ideals in regard to logic. One is the purely practical question of its use for every-day reading and thinking in the most important affairs of social and private life; the other is the epistemological question of the conditions for legitimate thinking in all problems, the higher philosophy as well as common affairs. The author does not seem able to cut himself loose from one or the other of these ideals, and hence produces a work that has to be expurgated for the beginner and expanded for the advanced student. This may, however, be a defect that it is hard to correct. The line between the theoretical and the practical is very difficult to draw, and much has to be left to the judgment of the man and to the mental conditions with which he has to deal.

When it comes to special points, having a purely logical and speculative interest, there are some opportunities for criticism. The definition of the subject seems to exclude mathematics from subordination to logic in the matter of method. Mr. Read excepts mathematical propositions from those for which logic provides a method of proof. Mathematical propositions he makes purely quantitative, and all other propositions qualitative. This distinction ignores the whole significance of the distribution of terms in which logic is given a quantitative meaning and definiteness, and if we could adopt Hamilton's quantification of the predicate we could make formal logic approximate mathematics much more than at present. The author's distinction can only mean either that logic is not a formal science at all or that the ratiocinative processes of mathematics are not subject to logic at all, as it has been defined in the past: namely, as a formal *scientia scientiarum.* Logic has all along been the organon of certitude (whether rightly or wrongly I do not need to say here), and as the reasoning of mathematics has always served as a model for certitude free from the liabilities of formal fallacies, there ought to be a most important connection between the two sciences, and this is the simpler universality of quantitative identity as compared with either the qualitative or with both quantitative and qualitative combined. Mathematical reasoning is thus the purest form in which the logical process can be embodied, and the distinction which fails to subordinate it to the determination of logic only makes the latter a material science coördinate with the others.

There is an interesting difficulty suggested by the author's doctrine of induction. He opens his discussion of this subject with the following statement: "Having now discussed terms, propositions, immediate and mediate inferences, and investigated the conditions of formal truth or consistency, we have next to consider the conditions of material truth: whether (or how far) it is possible to arrive at propositions that represent the course of nature and human life. Hitherto we have dealt with no sort of proof that gives any such assurance. A valid syllogism guarantees the truth of its conclusion, provided the premises be true; but what of the premises?"

This passage directly, and the absence of all discussion of the formal process of induction indirectly, shows that Mr. Read regards induction as a *material* process, and deduction as *formal.* The influence of Mill is very apparent in this view, and it opens the author's doctrine to all the objections that have been raised against Mill's theory. If induction be a process of reasoning at all, we ought to find a formal element in it, or refuse to separate the formal from the ma-

terial element in deduction. It is a defect in many of the Logics that they do not give sufficient attention to the formal aspect of induction. The interest of Bacon in material truths and scientific method has given a false conception to induction generally, especially as a formal process of reasoning, and our logicians seem unable to correct the error. The whole tendency of the doctrine that makes deduction only formal and dependent upon induction for the matter of its premises either assumes that reasoning is an ultimate source of knowledge, which is contrary to Mr. Read's own confession, or it has no right to admit material elements in deduction. If logic be a formal science at all, as, perhaps, all are agreed, both induction and deduction must be formal processes, and the source of material truth or knowledge must be wholly extra-logical, that is extra-ratiocinative. But the formal process of induction still awaits the adequate treatment which deduction has received. If the author means to regard deduction as the formal aspect of induction, he can maintain that there is only one form of reasoning and that the traditional difference between induction and deduction is that between matter and form. How far this is from the general conception of the case is apparent from the statement of it. There is some excuse for the author's view, but it can be maintained only by developing a new view of the processes in both deduction and induction, in which we shall find that the formal aspect may partake of two systems of distinct rules: namely, the quantitative and the qualitative.

There is less in other parts of the work to provoke the critic's animadversion. Some objections to order and analysis might be raised, but a departure from the usual procedure sometimes has the advantage of evoking new points of view, and the teacher may welcome rather than criticise it. The subject of fallacies, I think, has not received so scientific a treatment as it should have. They should have been classified and the fundamental principle determining them shown in its proper relation to them. The treatment of inductive and deductive fallacies in the same connection is useful, but unless care is preserved there will be some confusion between the processes of induction and deduction themselves. On the whole, however, the work is an admirable one. The illustrations are so full that the student must be as much interested as profited by his reading. It combines the merits, in this respect, of Mill, Venn and Keynes.

JAMES H. HYSLOP.

Wesen u. Aufgabe der Sociologie: Eine Kritik der organischen Methode in der Sociologie. L. STEIN. Berlin, C. Reimer. 1898. Pp. 38.

This brochure is the reprint of a paper published in the 'Archiv für systematische Philosophie,' the paper itself being largely the reproduction of a communication to the Third Congress of Sociology, held in Paris, in July, 1897. As shown by the title, the paper deals with the critical and methodological problem of sociology. Dr. Stein's faith in the future of sociology is not shaken by the uncertainty that still prevails as to the character and limits of sociological investigation, nor by the conflicting tendencies as to the methods of research. The fierce struggle still raging among sociologists is not, according to Dr. Stein, a proof of impotence; but, on the contrary, a distinct mark of exuberant vitality. 'Um Mumien kämpft man nicht' (p. 3). In the midst of the fray Dr. Stein boldly takes his stand against the so-called 'organic' (I would rather call it 'metaphoric' or 'rhetorical') theory of society. He agrees above all with Professor Giddings and those others who consider sociology, not as an encyclopædic conglomeration of the special social sciences, cast into a chaotic unity, but as a 'general' science of social elements and first principles. This 'general' science does not annihilate the special social sciences, whose autonomy is justified by the possibility of viewing the social phenomenon (Menschliches Zusammenleben u. Zusammenwirken) from three different standpoints: (1) the 'ontological,' (2) the 'historical,' (3) the 'ethical' (normativ), pp. 5–6. Had the unlucky word 'sociology' not been invented 'social philosophy' would have been the right denomination for the new 'general' science of society, which is essentially philosophical and synthetical in character (p. 7). Dr. Stein accepts the traditional division of sociology into two separate branches, 'Social Statics' and 'Social Dynamics.' The former considers the coexistence of individuals whose coöperation for the achievement of common ends gives rise to a certain stability of action, to 'eine typisch sich wiederholende Regelmässigkeit' (p. 7). Its method is chiefly descriptive. The latter endeavors, on one hand, to determine genetically (entwicklungsgeschichtlich) the 'causes' of the social facts discovered in the statics, and, on the other hand, to foretell the action of those causes upon the future direction of social evolution (p. 8). The problem with which social statics deals is the determination of the social equilibrium, of the stability of social conditions, viz.: of the coexistence of qualities and modalities in the individuals assembled in society. Social dynamics aims at determin-

ing the succession of the 'actions' of the same individuals. From the category of constancy we pass into that of variability. The method which is entirely descriptive in social statics becomes historico-genetic in social dynamics.

This distinction, which dates from Comte, has given rise to misconceptions, owing to its having been taken as the expression of an original and irreducible duality in the subject-matter of sociology. The final end of social inquiry is, as Professor Giddings remarks, "the synthesis of static and kinetic principles" (p. 60, *Prin. of Soc.*), reproducing the living unity of the social fact. The distinction can, then, be conceived only as a means to that end, imposed by methodological exigencies. I wonder that a cautious thinker like Dr. Stein should not have thought it necessary to point out the true character of the distinction between social statics and dynamics, in order to avoid misapprehension. It would, perhaps, be better to eliminate entirely these two technical physical terms, chosen by Comte as an image and loosely used as such. For statics and dynamics we could substitute, respectively, 'descriptive' and 'genetic' sociology, a denomination which, without any possibility of misunderstanding, would admirably cover the field assigned by Dr. Stein to the two branches of sociology.

Dr. Stein strongly emphasizes the difference between 'natural' and 'historical' causation. There is no denying that the law of causality, as a cosmical law, holds good also in social phenomena. But there is sufficient reason for doubting whether we can ascertain a mechanical necessity and a strict and rigorous generality in social phenomena through the only two sources of inquiry at our disposal, statistics and the theory of probability. Dr. Stein carefully discriminates between the purely 'historical' and the 'social' facts. As to the former, he, with Dr. Simmel, admits it to be an essentially unique thing, an 'Einmaliges,' or ἅπαξ λεγόμενον, which cannot be explained by a law, for a law means repetition, and, the historical event being the result of innumerable contributions, it becomes impossible to conceive its identical repetition. But in social phenomena—meaning by these the 'collective' phenomena, *i. e.*, facts relating to groups or masses as opposed to the different ways of assertion of an individual personality—Dr. Stein detects a rhythm, a constancy of structure and a periodicity of repetitions (p. 10). But this rhythm ascertained for social phenomena does not express a rigorous causality, as does the rhythm shown by physical, chemical and biological phenomena. Statistics prove that *B* follows *A* only in a great number of cases, say

in most cases, but not always. Hence social causation is not, in Leibnizian terms, a 'vérité éternelle,' but a 'vérité de fait.' A social rhythm offers only rules, like a grammar, and rules which admit of many exceptions. Here, says Dr. Stein, lies the πρῶτον ψεῦδος of social dynamics. It is just on the ground of this irreducible difference between natural and social causation that Dr. Stein strongly opposes the so-called 'biological' method in sociology. When we try to force the social phenomena into the strait-jacket of physical or biological laws, we substitute for scientific explanations verbal entities, images, metaphors. The attempt to explain society in terms of a physical or biological interpretation is not precisely a novelty. The origin of the 'metaphor' of the social organism can be traced back to the ancient conception of macro- and microcosmos. Through the infatuation for a sociology built on strictly physico-biological lines, the metaphor has passed gradually into a strong analogy, the analogy into a parallelism, the parallelism into a formal identity (p. 14).

In his endeavor to show the inefficacy of the 'organic' method Dr. Stein is led to discuss at length the question of social causation, and to point out the true character of the so-called social laws which are, in reality, nothing more than empirical generalizations. A mechanical necessity can be discovered only from the biological side of the social fact, that is to say, in Dr. Stein's terms, 'für den Chemismus des menschlichen Zusammenlebens,' but never from the psychological aspects of the human 'Zusammenwirkens.' Necessity, in social phenomena is of a purely *teleological* character (p. 15). This is a very important point in Dr. Stein's position, and he develops it at length in the remaining pages of his interesting brochure. By teleological necessity is meant the fact that, certain ends (Zwecke) being given, certain means must be selected in order to secure their achievement. The law of teleological necessity, however, does not work if the individual is unable to seize the relationship between the means and the end. Mechanic necessity, on the contrary, acts upon the individual without leaving his will to play any part in the production of the phenomenon. Natural necessity deals with the relationship between cause and effect; teleological necessity includes the relationship between ends and means (p. 31). Inanimate nature knows no aims. Conscious beings only show a tendency to adapt their actions to ends, and the higher the degree of consciousness the more complex the adaptations (p. 35). If social necessity is not mechanical but teleological, if our actions are not subject to an external mechanical pressure but are determined by internal motives, society cannot be conceived as an 'organism,' but as

an organization. The 'organism'—says Dr. Stein—is the *unconscious*, the 'organization' is the *conscious* coöperation of the single elements of a given whole to a common end (p. 36).

I cannot follow the distinguished professor of the University of Berne in all his suggestive developments. The chief interest of his brochure lies in the fact that, in spite of prolixity and repetitions, it brings out, in a very clear way, the conception of social causation as incidental to teleological necessity. This is indeed the fundamental idea of Tarde's 'Logique Sociale.' For the French sociologist social evolution is a progressive systematization of inventions. For Dr. Stein it is a systematization of ends. But the starting point of the two conceptions cannot but be common. Ends, as well as inventions, presuppose the modifying power of thought upon social conditions. And how are the ends set forth? Undoubtedly exigencies of personal and social preservation and development concur in determining the quality of individual and social aims. But, to repeat the old Aristotelean distinction, wants only furnish the *matter*, while thought—*i. e.*, invention—gives the *form*, by creating the whole system of social institutions and social ideals. Invention is just the unseizable and incoercible element of social life, it is the datum which cannot be reduced under the iron rule of law. It is the 'unicum,' the 'Einmalige,' the purely historical fact, it is the element of social variation and the instrument of social selection. It is only when invention has been thrown into the social stream that it spreads by way of imitation, thus giving rise to a series of similar acts which, just on account of their similarity admits of calculation, and therefore can become a matter of statistical investigation. Thus, once more, through the work of independent thinkers, the theory of imitation finds a useful illustration and a suggestive commentary.

GUSTAVO TOSTI.

The Subconscious Self and Its Relation to Education and Health. LOUIS WALDSTEIN, M. D. Charles Scribner's Sons. 1897. Pp. 171. $1.25.

In listening to a conversation we receive not only impressions of the words uttered, but also of colors and temperature, of the sounds in the trees, the odor of flowers. The attention is, however, fixed upon the words; these we are conscious of, while all the other associated or accompanying impressions sink into a more or less complete state of oblivion. Thus the act of concentrating the attention whereby we become conscious of certain impressions necessitates the exclusion

of a great mass of accompanying impressions. These impressions, which constitute in any given act of attention the fringe of the narrow circle of consciousness, we call subconscious impressions. Subconscious impressions are far more numerous than those designated as conscious; hence their totality in the memory complex is so much greater. As the accumulated contents of memory govern our emotions, thoughts and actions, the subconscious memory must play a great part in our individual life.

Subconscious impressions are: (1) Dependent upon the degree of attention developed in connection with the senses; (2) they are persistent when those consciously received may have become inactive; (3) they are most readily recalled by other subconscious and, therefore, involuntary impressions; (4) they are not under the control of the will. The subconscious memory is observed: (1) In the period directly preceding sleep; (2) in the delirium of dream and of fever; (3) in the semi-consciousness of the senility; (4) in the fictitious existence of the deranged mind, and (5) in the hypnotic state.

Impressions of one sense may be recalled by another sense through subconscious transference. This is illustrated at length by an experiment upon Helen Keller, wherein she recognized by the sense of touch a negro melody which she used to *hear* upon her father's knee.

The lower, undeveloped senses contribute most richly to this subconscious order of impression, and Dr. Waldstein asks whether the senses of smell and of touch are not largely accountable for our moods and emotions. Thus a warm draught of air, charged with some exotic scent, may recall one's life in the South and the joy or sadness of those days.

Our religious beliefs are not originally developed by the conscious self. As, in the words of Jacobsen, 'The church bell has rung God into our soul,' so in maturer life, when we fail, as may be, to experience a conscious self in our worship, it is the activity of the subconscious that holds us to the early faith. Thus the ritual of the church performs a legitimate function in engaging and impressing the subconscious part of the mind, and it follows naturally that those churches have lost the fewest of their followers whose manner of worship has always been calculated to work to this end. Dr. Waldstein discusses also the function of the subconscious in art creation, which, if not always convincing, is at least highly interesting and suggestive.

The first years of the child's life are devoted to the reception of subconscious impressions. It is, therefore, most important that the sur-

roundings which form the temperament should be chosen with care. When the nervous constitution of the child indicates that reflexes and emotions will multiply, and when the imagination of the child shows premature activity, the persistent training of attentive observation and conscious memory is necessary. On the other hand, in cases of sluggish imagination and weak emotional vitality we should discourage 'conscious training' and stimulate the subconscious by vaguer and artistic impressions.

The child subconsciously imitates the repeated actions of the parent, which will account by a more easy and natural explanation than that of *hereditary tendencies* for certain peculiarities and failings of our nervous organization. This explanation, the author takes pains to show, has one practical advantage over the explanation by heredity; for while the predicate 'inherited' carries with it the admission of the unalterable and ineradicable, the explanation by subconscious imitation implies the permanent possibility of reform—what has been learned can also be unlearned. It is thus, practically speaking, a less pernicious as well as more natural method of explanation than the traditional explanation by hereditary tendencies.

In so short a notice it is impossible to do justice to the wealth of illustration and practical suggestion contained in 'The Subconscious Self.' It should also be a permanent contribution to the psychology of the unconscious.

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L'État mental des Mourants: Nouveaux Documents. CH. FÉRÉ. Revue Philosophique, XLV., 296–302. March, 1898.

The discussions of this subject by Egger and Sollier have been already reported in this REVIEW. They relate particularly to the exaltations of memory and the feeling of beatitude which often occur to persons in mortal peril. Egger, it will be remembered, explains the reminiscences psychologically, namely, as the reaction of the whole organized self against the idea of its extinction. Sollier explains the 'euphoria,' which he regards as more negative than positive, brought about physiologically, namely, as the result of analgesia and anæsthesia in the case of accidents, by distraction of attention, while the reminiscences, he thinks, are probably a secondary consequence, past images filling the void produced by the loss of sensibility. M. Féré, in the paper before us, gives a résumé of the discussion and of the criticisms on it by Binet in the last *Année psychologique.*

The cases discussed hitherto relate the phenomena to the idea of death. The cases reported by M. Féré of persons actually dying are of a sort to bring the psychical phenomena into direct relation with the physical. The first case is that of a man dying of consumption. After a hypodermic injection of ether administered while he is in a state of apparent unconsciousness, pulse and respiration being extremely feeble, the man, a native of Belgium, but long resident in Paris and habitually using French, opens his eyes and says something in Flemish which is not understood. He accordingly makes signs for paper, and writes in Flemish something relating to a small debt contracted fifteen years before, and in a few minutes after dies. The second is a similar case of a man dying of pulmonary phthisis. In the momentary recovery following the injection of the ether, and almost in the very instant of death, he turns to his wife and says, "You won't find the pin; the whole floor has been relaid." The allusion was to something that occurred eighteen years earlier. Féré explains these cases as due to the transitory increase of the nutrition of the brain caused by the ether. In the following cases there was no such stimulus. An old man was injured by a fall. The result was a succession of crises, accompanied by cerebral torpor of increasing depth. In his last attack there was no sign of recognition for two days; but just before his death he opened his eyes wide and, looking at a small picture on the wall, in brief phrase bequeathed it to an old friend from whom he had been separated by a trivial misunderstanding for twenty years. The fourth case is that of a woman suffering from uterine cancer. The progress of the disease is accompanied by convulsions and coma. At the end the coma had lasted for three days with not a sign of intelligence. A quarter of an hour before she died she remarked, what was true, although nobody else had noticed it, that the lamp was smoking. In all of these cases, cases in which the memory appears restricted and insignificant, Féré thinks that association with the idea of death is very doubtful. The facts naturally connect themselves with the momentary exaltations of psychical function often found in the insane and idiotic just before death, and, as also in normal persons, in the course of certain acute diseases. The explanation is physiological. The well-known prelethal hyperexcitability of animal tissues has recently been shown to apply to the cerebral cortex by Broca's and Richet's experiments on dogs. The psychical excitability corresponds to this physiological hyperexcitability. The euphoria, which is more positive than Sollier supposes, is explained by the same condition, pleasure and pain being in direct relation to the amount of disposable

energy. Where there are no artificial stimuli this hyperexcitability is probably caused by visceral and peripheral stimuli. In fine, the explanation of these cases is twofold: momentary hyperexcitability of nervous elements about to lose their attributes, and also the action of the products of waste matter on the nerve centres and on the peripheral nerves. Even where death does not ensue the explanation is the same, if the fear of death can establish in a certain measure the physical conditions of death.

It will be observed that this explanation, which certainly appears to advance our understanding of the facts, does not touch the question of their specific character. This in the last case cited seems to have been due to an actual olfactory stimulus. In two of the others, the first and the third, the connection with the idea of death does not seem so absolutely excluded as Féré supposes. The remarks have the appearance of a final settlement of conscience, and the same might be found to be true, if we knew all the circumstances, in the remaining case. As to the explanation suggested for the feeling of beatitude, it is clear that in none of the cases is there any evidence that it existed.

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Untersuchungen an total Farbenblinden. Von PROFESSOR C. HESS, in Marburg, und PROFESSOR E. HERING, in Leipzig. Pflüger's Archiv, 71, 105–127. 1898.

The investigations about to be described were undertaken principally for the purpose of subjecting to an experimental test the views lately brought forward by Ladd Franklin, A. König and especially v. Kries, for the explanation of total color-blindness. Three persons were experimented upon—the same lady who was formerly examined by Herr Geheimer Rath von Hippel and (to some extent) her older sister, and also a young girl at Berne, who was tested by Professor Hess, and who will be more fully reported on later by Professor Pflüger.

The diversity in function of the rods and the cones which has lately been so much in the foreground was already suggested by Kühne and by Max Schultze. Kühne said that it was probable that if we had no cones we should 'see the spectrum in shaded grays, like the color-blind.' Since this assumption does not fit well with the Young-Helmholtz theory, v. Kries, who still adheres to that theory, now makes the additional assumption that there are two kinds of white-sensation—one furnished by the rods, which give no color-sensation, and one by the

cones, which latter only is of the nature of the tri-chromatic white-sensation of the Young-Helmholtz theory; and König holds a similar view to this. The distinctive feature of what may be called the hypothesis of v. Kries is, therefore, the existence of these two different sorts of white (unless König should lay claim to priority in this view). Two different sorts of white are not required by the others who have supported the doctrine. "If, as Max Schultze suggested, the production of the colorless sensation is a function common to the rods and cones, and the color sensation is exclusively brought about by the functionally farther differentiated cones, then we might suppose (as Ladd Franklin has already done) that in the totally color-blind the cone-apparatus has remained, in respect of what it can accomplish, in the stage of development of the rod-apparatus, and that it, too, can only produce the colorless sensation, in which case a central blind spot would not need to be present. Another supposition, and one that also excludes a central blind spot, would be that the cone-apparatus is not even rudimentarily developed, and that the layer of the visual cells is represented by an uninterrupted series of rods. But the hypothesis that the cones are present, but that they are incapable, from birth, of performing their function, will appear to the biologist quite as strange as to suppose that they are wanting altogether 'while [in the words of v. Kries] the spatial distribution of the rods coincides with the norm.'" Inconsistently with the hypothesis of v. Kries, Hering finds that in the case of the color-blind person now examined no central blind spot could be detected. Though she had formerly had nystagmus, she was now free from it, except when she looked far to one side; she could fixate well, and the fixation-point of her retina was at the normal distance from the blind spot; she had no difficulty in obtaining the entoptic vision of the macula; and when a number of small, bright spots were presented to her it did not happen that one and another of them constantly disappeared as her eye wandered over the group. [This last consideration is conclusive, provided the size of the spots and their distances apart were properly chosen. The others would not be so: a permanent substitution fixation-point is often adopted at one side of the fovea, and in such a case there would be no nystagmus; this spot might easily be above or below the fovea, and then its distance from the entrance of the optic nerve would not differ perceptibly from what is normal; and since the central blind spot would be much smaller than the macula, it might easily be overlooked within it.]

Since this color-blind individual seems to have had perfect vision

in the fovea, she must have had, if not cones, then rods there. But if she had rods in the fovea, since the rods are the seat of the so-called visual purple, and that is the means of faint-light adaptation, she ought to have had, unlike people of normal vision, good adaptation in the fovea, that is, the normal night-blindness of the fovea ought to have been wanting in her eyes. That turned out not to be the case; in a faint light small objects disappeared when their images slipped into her fovea, exactly as for the normal eye. From this it follows, according to Hess and Hering, that her central vision cannot have been rod-vision, and hence that total color-blindness, in general, cannot be due to a total lack of functioning on the part of the cones.

The occasion was taken advantage of to make a number of other observations on this case of defective vision. The patient was only slightly discomforted by a bright light at this time; formerly, when tested by v. Hippel, it was very disagreeable to her, and caused the sensation of a white mist before her eyes; now, that sensation was produced only by direct sunlight. v. Kries determined the visual acuteness for the rods in his own eye by testing with the Snellen types in a faint light, and found it to agree with that of the totally color-blind; this consideration would be of weight if it was shown that the totally color-blind see no better at their *optimum* illumination than at the faint light at which v. Kries tested his own eyes, but Hering and Hess found that their visual acuity is diminished when the illumination is faint, and hence the coincidence has not much significance. The power of discrimination for separate points diminished from the fovea outward, as in the normal eye, and there was nothing peculiar about successive and simultaneous contrast, nor about the perception of brightness differences, nor about the after-image of rapidly moving points.

There is, therefore, for two distinct reasons, nothing in the observations here reported upon which goes against the hypothesis, taken in itself, that total color-blindness is, in many cases, due to an incomplete development of the cones; and still less is there anything to diminish the probability of the distinction of function of the rods and the cones. But the case in rebuttal is not equally strong for the curious theory of the light sense which is now defended by v. Kries. That writer accepts for the rods a purely physiological sensation of white, due to a definite and unitary photo-chemical process; and side by side with it, in the cones, a purely imaginary sensation, of indistinguishable quality from that, but caused by the inability of the human mind to know that it is seeing the three colors red, green and

blue all at once, if it is seeing them in (in some sense) equal amounts. This view is in itself so indefensible on the ground of ordinary common sense that it looks like rather a waste of ammunition to attack it with experiments. But while the argument here brought out against that theory is not wholly necessary, neither is it thoroughly conclusive. It is true that it is not permitted to explain the anomalous cases of total color-blindness here described as due to defects in the optic nerve or in the higher centers; for if there is any difficulty in the transmission of the color process from retina to brain, then the sense for the mixed white of the fovea would vanish at the same time as the sense for color. But, as we have pointed out, there is nothing in the way of supposing that there are rods in the fovea which are lacking in the visual purple which is characteristic of the rods of the mid-periphery. With this supposition, the theory of v. Kries is not much worse off than it was before.

C. LADD FRANKLIN.

Ueber Raddrehung, Rollung und Aberration. A. MEINONG. Zeitschrift für Psychologie und Physiologie der Sinnesorgane. Bd. XVII., Hf. 3, p. 161.

Discussions of the laws of ocular movements are always difficult, and often obscure, when they come to deal with the movements of torsion. The treatments of this part of the subject by different authorities have resulted in conclusions that are not readily comparable, and frequently these conclusions seem to be contradictory. Meinong has succeeded in this article in clearing up the matter by defining more fully the various aspects of such movements, and by assigning to each, as its distinguishing name, one of the terms that have been used more or less loosely as synonyms to refer to the movement in general. English terminology is in this case no clearer than the German, and this is a favorable opportunity for improvement by adopting, as far as possible, terms equivalent to those proposed by Meinong.

Movements of the eye may be defined according to their axes as follows: first, movement about a horizontal axis; second, movement about a perpendicular axis; and third, movement about the line of regard as an axis. Other complex movements can be reduced to these three as component movements. If, now, any actual movement contains a component belonging to the third class we have in this component a true movement of (*Rollung*) torsion. In certain complex movements, as, for example, in movements from the primary position, torsion is not present. If, however, instead of passing directly from

the primary position to a point situated obliquely above or below, the eye moves first on its perpendicular, and then on its horizontal axis, the latter movement will be accompanied by torsion.

From this movement of torsion are to be distinguished two changes in the relative position of the retina. The first is a deviation of the perpendicular meridian of the retina from the objective perpendicular. This failure of the perpendicular meridian of the retina to correspond to the objective may be designated (*Aberration*) aberration. It may appear, as, for example, when the eye is moved obliquely from the primary position, even though no torsion has taken place. Finally, if in any position of the eye the planes of regard and of the retinal horizon do not coincide, the angle between them may be called the angle of (*Raddrehung*) rotation. The term torsion applies, then, to actual movement, while the terms aberration and rotation refer to angles of position.

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Zur Theorie der räumlichen Gesichtswahrnehmungen. Von W. WUNDT. Philosophische Studien, Vol. 14, pp. 1–118.

The question as to the origin of the spatial character of sight is here treated in an article which is indeed attractive and certain to stimulate considerable discussion. The mode of procedure is, in the main, strictly empirical; the author devotes himself to a leisurely review of certain facts which seem to him to put us in a position where a decisive answer can be given to the problem in hand.

As part of the data on which his conclusions are based, Wundt gives the history of his own remarkable case of retinal disturbance, due to a diseased condition of the underlying tissues (*choroiditis disseminata*). Irregular displacements of the retinal elements occurred and were accompanied by an apparent distortion of objects in view. In time, however, this distortion passed away, and while, as Wundt points out, it is not impossible that the displaced parts may have returned to their original positions, this seems, under the circumstances, highly improbable. The indications were that these parts remained displaced and finally became functionally adapted to their new positions. For it is evident from other cases, where a persistent refractive instead of a retinal disturbance has existed, that a functional readjustment can take place in the retina, and the earlier irregularity in the appearance of things entirely disappear. There is, therefore, no hard and fast spatial meaning attached to a particular retinal element;

and since an element can thus acquire a new spatial meaning, there is every reason to suppose that its primary local signification was also acquired and not received as a native endowment.

The more important experiments on the perception of depth are next discussed, with especial emphasis on the evidence that these perceptions arise, not from the will nor from the differences in the two retinal impressions, but from a complex of extra-retinal sensations originating chiefly in the sockets of the eyes, although strictly muscular sensations may play an important subordinate rôle.

But by far the weightiest facts, the author believes, are furnished by the so-called geometric optical illusions. The well-known deceptions in the length and direction of lines show the influence of the eyes' motor mechanism in our space-perceptions—an influence still more strikingly revealed in those ambiguous outlines, such as Schroeder's stair-figure, which show a reversible perspective. In this latter class of illusions, according to the author, the actual relief which the outline assumes depends solely on the point of fixation or (if we look, not fixedly, but glancing) upon the direction in which our eyes move along the lines; if one fixes or moves his eyes thus and so, the figure appears in a certain relief; if he does otherwise, the perspective is reversed. The slight variation in the position or movement of the eye here makes the perception take on an entirely different appearance. This, Wundt feels, is most significant for a theory of spatial vision.

After examining this evidence the author concludes that the motives for visual localization are neither of retinal origin exclusively nor merely from the tactual and muscular function of the eyes, but are to be found in an intimate fusion of sensations from both of these sources; each 'local sign' is a composite of elements coming from both the retina and the tactual and muscular parts. This he calls the 'theory of complex local signs.' In stating his theory Wundt takes a stand, moreover, against nativism, inasmuch as the spatial aspect of vision is a gradual development under definite empirical conditions. At the same time, he objects to calling his own an empiristic view, because space, psychologically speaking, cannot justly be said to result from 'experience.' The spatial quality in our mental life is certainly a product, but a product which must precede experience in any proper sense, and, therefore, cannot flow from it. He prefers the term 'genetic' to express this side of his doctrine.

The facts which Professor Wundt brings out seem sufficient to convince one that extra-retinal sensations have an influence on the

total visual result. During the reading one feels that this has been so abundantly supported that more ought to have been said for the other side of the theory, namely, that each local sign has also a retinal factor in its composition. The author has nearly left us to our own resources in regard to this, whereas at the present day it is exactly here that doubts are most ready to assail.

This uneven distribution of the author's forces is perhaps best to be explained by the fact that his object seems to have been, not so much to marshal all the data which make for his view, as to point out the significance of certain interesting phenomena, more especially the illusions of reversible perspective. Whether these illusions may really be interpreted as Wundt believes, can hardly be decided without more extended experiment. As far as my own observations go, the sudden reversals do not follow the simple laws of ocular position and movement which the author states. Rhythmic alterations of relief occur in my case when the point of fixation has apparently remained unmoved, and even when—as in observing not the figure itself, but its after-image, the diagram which Wundt counts the best having first been retraced white on a black ground, and exposed to a strong light—any movement of the fixation point, at least with reference to the image itself, was impossible. Likewise, repeated movements of the eyes along a given line do not in my case give constant results. If such irregularities are at all frequent they will certainly be an obstacle to our drawing from these illusions the inferences Wundt does.

The subordinate importance which Professor Wundt gives to sensations from the eye-muscles as compared with those produced by pressure and friction in the sensitive sockets will be welcomed by many who do not feel that *Muskelempfindungen* explain all things.

As for the general theory of visual extension proposed, it is perhaps the one which on the whole seems best to accord with the known facts. One can hardly say, however, that the facts adduced are absolutely coercive against the opposing views that the spatial perception may under certain circumstances be mediated by local signs which are not 'complex' in Wundt's sense. All we can say is that in certain perceptions the motor as well as the purely retinal impression is psychically operative. But that in every instance both factors are absolutely indispensable may perhaps still be counted an open question.

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Die geometrisch-optischen Täuschungen als Nachwirkungen der im körperlichen Sehen erworbenen Erfahrung. WILH. FILEHNE. Zeitsch. f. Psych. u. Physiol. d. Sinnesorg. XVII., 15. 1898.

The above article evidences once more the lively interest now taken in the general subject of optical illusions. This is not a systematic treatment of the whole field, after the manner of Thiéry, Lipps and Wundt, but it is a most interesting attempt to apply the perspective interpretation to a few of the best-known illusions. The general thesis of the author is clearly enough indicated in the above title. It is in effect this: Our visual experience is preponderatingly tri-dimensional. Ordinarily we see solids, edges and corners, not mere plane surfaces with lines and points. In paintings, photographs, etc., we have the same experience. Consequently, when we see mere linear drawings our visual habits gained from ordinary experience compel us, as it were, to give them a three-dimensional interpretation. We are, indeed, for the most part unconscious of the particular perspective motives that exert this compulsion in any special case. But the immense stock of memory-images, acquired in the course of spatial experience, are here half awakened by the presence of the lines, and, though remaining partly below the threshold of consciousness, are still effective in accomplishing their end. The influence of this 'half awakened, otherwise latent store of memory-images' the author attempts to show in the case of simple diagrams, to which the reader must be referred.

Thus far we have nothing new. The theory of the author does not differ essentially from that of Thiéry, so far as the present writer is aware. The *novel* feature of the article is the endeavor to suggest certain actual experiences in every-day life that may be taken as typical of that mass of experience to the influence of which optical illusions are due. Here we find some most interesting material. Consider, *e. g.*, the lines of pictures, which regarded by themselves, apart from their meaning in the picture, are either parallel or continuous. If *parallel*, they may represent lines diverging to any extent. For example, a picture presents a roadway along which runs a telegraph line. If, now, a log lying directly across the road be introduced, it will be *seen* as perpendicular to the telegraph line, but must be *drawn* parallel to it. Again, lines actually *continuous* in a picture may stand for very diverse objects. The drawing of a square umbrella-stand may portray an ornament pointing in one direction from its corner and an umbrella in the stand pointing away in quite a different direction. Yet ornament and umbrella may be depicted by broken

parts of one and the same straight line. The tri-dimensional *meaning* of the lines determines their separation to different planes of space.

Before reporting the author's treatment of particular illusions it should be said that he brings the supposed over-estimation of acute angles and under-estimation of obtuse angles under the same perspective principles. They are falsely estimated *only when* perspective suggestions are present. A simple angle, as such, is neither over-estimated nor under-estimated. Angles lying in the vicinity of 45°, 90° and 135° and varying in both directions by stages of ½° are drawn and submitted to observers. The latter are required to select a right angle, the half of a right angle and a right angle and a half. Errors, never over 3°, are made *in both directions* and with about the same degree of frequency in each direction. The false estimation usually spoken of occurs then only in the presence of perspective motives, and here even a right angle may be seen as other than it is. Heyman's comments upon this matter, supported by measurements of the axes of ellipses, miss the mark altogether, since a mere ellipse, as such, has no perspective motive. But let the latter be added by the drawing of two parallels perpendicular to the longer axis at its extremities, and the shorter axis immediately appears longer, since the ellipse now represents a circle.

Four illusions are discussed vith a view to suggesting the actual experiences instrumental in producing them—those of Poggendorff, Münsterberg, Zöllner and Loeb.

1. The Poggendorff illusion. According to the author, this illusion is due to the fact that the two ends of the transversal, sundered by the vertical strip, are made to belong to two different systems, each suggesting its own perspective interpretation and being under no necessity, consequently, of appearing as parts of a continuous line. This is clear to the author from the consideration that, if accessory lines be drawn in the figure in such a way that the two ends of the transversal are seen manifestly to belong together, the illusion vanishes. This can be secured, we are told, in various ways: by making the transversal represent a pointed stick; by introducing at the outer ends of the transversal something to arouse the idea that the line as a whole is being stretched; or, best of all, by uniting the two edges of the vertical strip by a line which shall lie obliquely within the strip and meet its edges at points opposite those in which the ends of the transversal terminate. Each one of these devices accomplishes the union of the two sides of the figure, and the lines appear continuous, in the last case, indeed, even when the drawing is viewed as a plane figure.

Now, it may be said at once that for the present writer and his observers the illusion does *not* disappear in these cases. Under the last mentioned conditions a simple contrivance for the quantitative study of the figure reveals in every instance the presence of the usual illusory displacement. Then, too, one at all familiar with the history of the illusion recalls at once that it has been many times independently discovered in the form of the Gothic Arch Illusion under conditions of normal vision, where not merely perspective motives, but perspective realities, were present. The author thinks differently, however, and suggests that such visual experiences as may be given by the finger-boards of the German railway stations furnish the store of memory images which make the two sundered portions of a line appear independent the one of the other. The reader must be referred to Fig. 22 of the text, which presents this thought in a most picturesque way.

2. The Münsterberg illusion. This is called the Milton Bradley figure in the text, and is the now well-known 'illusion of the shifted checker-board figure.' The author attempts to apply his perspective theory here by endeavoring to show that the rows of squares held horizontally suggest a bench, one end of which recedes from the observer. If, now, the line between the squares be made to converge towards the base line, and the squares supposed to be in the distance be made smaller, the illusion is destroyed. Criticism of this attempt is hardly called for, partly because the author's procedure here is wholly arbitrary, but chiefly because it has been shown in a recent number of this REVIEW that the illusion in question is due solely to *irradiation*.

3. Zöllner's pattern. The author's service here lies in the construction of a model whereby the grounds for the well-known plastic phenomena are made clear. On a horizontal plane are placed parallel pairs of uprights of gradually increasing heights. Rods crossed by short perpendicular wires are placed with their ends in these uprights. The rods are so tilted that the nearer ends are alternately higher and lower from the plane than the farther ends. The perpendicular cross-lines also slope alternately to either side. Viewed from above, this system of lines is projected upon the horizontal plane in the form of a regular Zöllner pattern. The main lines are parallel and the angles made by the cross-lines are seen as oblique. Yet the perspective interpretation of this projection is the model itself, which presents in reality the phenomena of ridges and lines projecting from the plane to which Hering and Guye long ago called attention. As a type of the actual experience from which influential memory-images may have

come, we are given the sketch of a long factory-building of four sections, the lines where the sloping roofs meet above and below forming the main lines, others parallel to the gable edges of the roof forming the oblique lines of a Zöllner figure. In such *real* cases as this the parallels are, of course, represented by converging lines; consequently in the Zöllner pattern the lines, actually parallel, must appear to diverge.

The general position of the author is considerably fortified by certain new observations which he reports in respect to illusory *movements* seen on the heavy-line pattern of Zöllner. These movements are similar in character to those noticed by Helmholtz when fixating a pin-head moving across the diagram. They may be seen by viewing this heavy-line diagram through a tube, the most favorable position being that in which the main lines are horizontal. These lines, with their obliques, are then seen to dart to right and left *always in the direction of the overhanging ends of the obliques*. Similarly for the vertical position of the main lines. But now a curious fact. If the figure be so turned that the main lines slope upwards to the left the movement is particularly lively. If, however, they slope upwards to the right no movement is seen. The two diagonals of the figure are not equally significant. This arises from the fact that in the first case the horizontal and vertical movements reinforce one another; in the second case they are antagonistic and annul one another. But why any movement at all? This is answered by appeal to 'memory-images of motion.' The latter have their source in experiences of which the following is perhaps the simplest example. As we ride in a railway car and watch the passage of telegraph poles across the field of view we note that these appear to rotate as they pass, always in a direction opposite to that of the train's motion. We notice, too, that as any pole recedes from us it appears to tip more and more away from us, making a greater and greater obtuse angle with the plane of the roadbed. That is, when we move past perpendicular objects they seem to fly by us in the direction of that position where they seem to fall more and more forwards. But this is exactly what happens in the Zöllner pattern. The lines seem to move forwards in the direction of the overhanging ends of the obliques, a phenomenon due obviously to such real experiences as the above. The author's observations here are by no means easy to verify. The movements are very elusive and sometimes do not occur at all. The writer finds help in slight movements of starting to close and open the eyes, but the author assures us that eye-movements are not necessary. The whole matter is so interesting that these illusory movements, together with those noticed by Helmholtz, need further investigation.

4. Loeb's illusion. The destruction of perspective motives and the consequent disappearance of the illusion may be accomplished here in several ways. The strips or coins may be placed on the inner side of a cylindrical surface, in the axis of which the eye is placed so that all three elements of the figure are equally distant from it. Or tiny parallelopipeds may be used on a plane surface, these not admitting a perspective interpretation. Or, best of all, the strips may be replaced by three large I's, about which carefully arranged and legible print is laid. Reading the latter (in indirect vision, of course) destroys the illusion, because everything present must be interpreted as lying in one plane. Exclusive attention to the strips permits the return of the illusion, but at the same time one can observe a change in the accommodation, which would indicate the adaptation of the eye to tri-dimensional conditions. The experiences to which the illusion is due are somewhat hard to specify, but, as the diagrams of the text show, they occur whenever in architecture or in gardens posts and other cases uprights are so arranged that, while actually standing at points unrelated to each other, they must be represented in any drawing by one and the same straight line. The explanation is ingenious, but not wholly satisfactory in view of Wundt's recent discussion, and inapplicable, it would seem, to that form given to the illusion by Sanford.

In the present state of the question it is perhaps impossible to take a decided stand in reference to the part played by perspective influences in the production of any given optical illusion. This principle of explanation is certainly not universally applicable. But just as little, it seems, is the opposite course justifiable, namely, to make a wholesale relegation of perspective to a secondary position, as Wundt has done. To establish a multiplicity of explanatory grounds seems rather to be the course likely to prevail in the future. In any case, it is manifest that the last word about the matter remains yet to be said; meanwhile a distinct contribution has been made by the article that we have considered.

A. H. Pierce.

Amherst College.

Emozioni e sogni. Sante de Sanctis. (Repr. fr. Riv. Sperim. di Freniat.) Reggio-Emilia, S. Calderini e Figlio. 1896.

Experiments were made by the author on the relation between the emotions of waking life and those of sleep. The subjects examined were 200 normal persons (including 50 women), 125 criminals (including 24 women), 43 prostitutes, and a large number of pathological

cases of various sorts; observations were also made on a number of babies and animals. Four distinct categories were found. In the first there was no evidence that any relation existed between the emotions of waking life and those of dream life; this category embraced about 40 per cent. of the normal adults (nearly all light dreamers) and 70 per cent. of the pathological cases. Among a second class of persons a connection was sometimes observed, sometimes not, according to the nature of the emotion itself; *e. g.*, fear would be reproduced in sleep, joy would not, etc. This class contained 16 per cent. of the normal and 26 per cent. of the pathological cases. In the third class the emotions of waking life were generally reproduced in sleep, and retained their original character; this included 44 per cent. of the normal cases and a number of the pathological. The fourth class consisted of cases where the emotions were reproduced with quality inverted; *e. g.*, hunger in waking life would lead to dreams of its satisfaction, etc. Traces of this were found in some of the pathological cases, in but two of the normal subjects, etc.; no traces were found in children or animals. The investigation touched upon the converse problem: the continuance of dream emotions after waking. The phenomenon was noted in about 50 per cent. of the normal subjects, among the pathological cases generally, frequently among the children observed, but rarely among the animals.

From the results, the author concludes (1) that, in order to be reproduced in dreams, an emotion must be, not merely an intellectual or simple reflex phenomenon, but something 'intimately felt by the subject.' (2) It must be of moderate intensity; if too strong, its force is exhausted in waking life and it is only reproduced in sleep with difficulty, or not till long after; if too weak, it disappears quickly and is lost. (3) The emotional life runs a course of its own, and is not necessarily controlled by the train of intellectual consciousness. In extreme cases there seems to be, as Ribot says, a purely effective memory distinct from the purely sensational memory; in many persons the dream memory is preëminently, or exclusively, emotional. (4) Continuity of the emotional life from normal to dream consciousness, and *vice versa*, seems to be a phenomenon of low psychogenetic order. The more advanced an individual is, intellectually, the more the waking consciousness differs from that of dream life, the latter becoming more or less atavistic. Where the culture-stage is low, and among children, the two forms of consciousness are similar. In the latter case the dream consciousness is less likely to make an impression and be remembered than where the two are markedly dissimilar.

—The author discusses the views of other writers at length, and gives a good summary of the literature bearing on his line of investigation.

H. C. WARREN.

Modifcazione delle percezioni visive sotta l'influenza di sensazioni gustative simultanee. Ricerche sperimentali su adulti e bambini. S. DE SANCTIS and B. VESPA. (Repr. fr. Riv. di Psicologia, etc.) Rome, Frat. Capaccini. 1898.

Following Féré's suggestion regarding the dynamogenic influence of acid tastes on other sensations, the authors made a series of experiments on the changes produced by salt, acid, sweet and bitter flavors on the perimetric extent of the field of vision. Their subjects consisted of 2 normal adults, 8 children and 3 pathological cases—all of whom, with one exception, possessed a normal perimetric range of vision. Although the experiments were scarcely numerous enough to warrant generalization, the results seemed to point clearly to an inhibitive effect of gustatory stimuli; in a majority of cases the field of vision was contracted under the influence of all the flavors, sometimes to a very marked degree. This negative dynamogenic effect was considerably greater among the children than among the adults tested. The phenomenon appeared to be primarily a function of the attention, rather than of sensation, the gustatory stimulus, especially when strong, having the effect of distracting the attention from the visual centers; through the development of voluntary control of the attention, the adults were better able than the children to resist this influence and concentrate their attention upon the visual centers.

H. C. WARREN.

Ricerche sperimentali sulla memoria musicale nei frenastenici C. BENARDINI and G. C. FERRARI. (Repr. fr. Riv. Sperim. di Freniat.) Reggio-Emilia, S. Calderini e Figilio. 1896.

Le facoltà musicali e le loro alterazioni secondo gli studi più recenti. G. C. FERRARI. (Repr. fr. Riv. Sperim. di Freniat.) Reggio-Emilia, S. Calderini e Figlio. 1896.

The authors tested 100 patients of the Psychiatrical Institute at Reggio, with respect to their memory for tunes. One of the investigators sang a simple musical phrase, and the subject was asked to repeat it; the process was continued (where any degree of success was obtained) until the tune was memorized completely. The major chord was first tried, then two other simple airs, with which the patient was

not familiar. The second investigator meanwhile took account of the number of repetitions, the degree of success, and any noteworthy circumstances attending the experiment. Those who succeeded in the task were recalled some 20 or 30 days later, and asked to repeat what they remembered of the former test; at the same time three new airs were taught them in the same manner as before, for purposes of comparison.

The results showed three classes of subjects: 1. Those of good musical ability and memory, 12%. 2. Those who showed signs of possessing some musical memory, but who were slow in learning, and scarcely ever remembered the airs from one time to another, 20%. 3. Those who were either obstinate, 14%, or who were inattentive or incapable of memorizing, 30%, or who remembered only the rhythm, 9%, or only the rhythm and a few notes, 7%. The eight remaining subjects were mutes, and resisted every test. (The authors' results give less favorable percentages than those obtained by Wildermuth in his investigation of the musical capacity of idiots.) In very few cases was the repetition exact; after 4–7 trials the best subjects generally caught the phrase with some variations, which were not always in the nature of simplifications; these were preserved thereafter (whether noticed by the subject or not), and recurred in the same form at the second sitting. The authors promise a more extensive report on the problem when they have had an opportunity to examine a larger number of subjects.

In the second paper Dr. Ferrari gives a review of theories regarding the nature of the musical faculty and its diseases, from the time of Gall down to the present. He observes that ideas of music are of various types: visual, auditory, motor and mixed. This would seem to make the musical consciousness depend in different cases on different association tracts, and would account for the several distinct types of amusia. The author believes that the classifications of Knoblauch, Wallaschek and Brazier are too complex and artificial. He himself divides amusia into two general classes: sensory (including visual and auditory types), and motor (including vocal and instrumental). Cases of each are cited; the evidence seems to show that sensory amusia is possible without motor disturbance, as well as the opposite. In conclusion Dr. Ferrari discusses the positions taken by different writers as to the therapeutic value of music. Even at the present time many prominent physicians accord it a high place in the treatment of nervous diseases.

H. C. WARREN.

Ricerche ergografiche nella donna. G. C. Ferrari. (Repr. fr. Riv. Sper. di Fren.) Reggio-Emilia, Calderini e Figlio. 1898.

Tests were made with Mosso's ergograph of the comparative rate and manner of fatigue for the two hands. Of 14 women (6 normal, 5 ordinary patients and 3 hysterical), none of whom was left-handed in practice or by the dynamometric test, 10 proved to have greater endurance in the left hand, 1 was ambidextrous, and only 3 were right-handed, by the ergograph test. On the contrary, of 14 men (11 normal and 3 mental patients), none left-handed by the dynamometer, only 2 were 'ergographically left-handed' and 2 ambidextrous. The author observes two distinct types among the ergographically left-handed. In some cases the two hands preserve same relations throughout the tests, whatever the weight used; the number of strokes and the variations in their height are the same; but the left hand employs each time a greater force, so that its strokes are uniformly longer. In the other type, less common, but more characteristic, the first two tests give similar results for the two hands, but as the weight is increased a marked difference arises in the number of strokes and their height.

Although the number of cases reported is small the author believes them to be representative. He considers them evidence of an essential difference of character between the two cerebral hemispheres of woman; while the left is the seat of speech and other intellectual functions, 'the right is the repository of an hereditary, unorganized force of passive resistance.' The hemispheres are less closely coördinated in woman than in man. Thus, in woman, while the tests of the right hand (left hemisphere) give the measure of *psychological* fatigue, those of the left hand (right hemisphere) are rather a measure of *physiological* fatigue. The former are comparable with the similar tests in man; the latter give results which belong exclusively to the female sex. The theory is of so radical a nature that it deserves to be tested by a much larger number of instances.

H. C. Warren.

Some Judgments on the Size of Familiar Objects. Am. Journ. Psych., IX., 2, p. 137.

This is a simple but concise and valuable study of constant errors in memory of the size of objects with reference to age and sex. School children of the fourth and eighth grades and university students were examined. They were asked to represent the size of a silver dollar and each of the fractional coins by circles, and a five-dol-

lar bill by a figure of its shape and size. They were also required to draw plane figures with dimensions stated in inches, to reproduce the combined areas of these, and to estimate the dimensions of the paper used. To children a memory image is smaller than its object. The under-estimation of the coins and the bill by the younger children is enormous. The size of the dime, the nickel and the bill is also under-estimated by the adults. The degree of accuracy increases with grade. Grade is a better basis for classification than age. There are some characteristic fluctuations with sex, but on the whole there is no marked difference in the ability of boys and girls in this test. More attention should be given to perception of size in drawing.

C. E. SEASHORE.

A Contribution to the Study of Illusions. FREDERICK E. BOLTON. Am. Journ. Psych., IX., 2, p. 167.

The first part deals with 'the effect of size upon the estimation of weight.' Of the two series of blocks used, one varied in weight and the other in size, the standard being 55g. First, the blocks in the former series were matched by those in the latter (1) by pressing levers on which the blocks were balanced (*a*) out of sight, and (*b*) in sight, and (2) by lifting them between the fingers in sight. Then the process was reversed and the blocks that differed in weight were matched by those that differed in size, under similar conditions. The usual size-weight illusion occurs. Referring to the two methods, the effect of weight upon size is greater than the effect of size upon weight. The 'pressure' method gives a much larger mean variation and a smaller illusion. By the pressure method there is also an 'unexplainable' tendency to under-estimate the weight of the block compared.

The second part is a study of 'the effect of contour upon area.' The problem is, "Does any illusion of area ensue when the contour of a given surface is modified?" The general reply is negative.

C. E. SEASHORE.

Dreidimensionale Analyse von Ausdrucksbewegungen. R. SOMMER. Zeitsch. f. Psychol., XVI., 4. Pp. 275–297.

The importance both for normal and abnormal psychology of the study of our involuntary muscular movements has long been acknowledged, but, unfortunately, the invention of instruments for the analysis of these movements has not kept pace with the demand. Every one who has worked with the planchette knows the difficulty, almost impossibility, of unraveling the record obtained from it as usually used,

and here one deals only with two directions and the extent of the movement. Professor Sommer is, therefore, to be congratulated the more for succeeding in constructing an instrument that permits not only a careful tri-dimensional analysis of a movement, but at the same time shows its duration and extent. Briefly described, the instrument consists of a stirrup for one or two fingers, connecting by compound levers with three styles which respectively mark on the revolving drum of a kymograph all movements, right and left, up and down, to and fro. The pen levers greatly magnify the movement, so that even the slightest tremors are shown. Several typical curves of reactions show some results obtained, and the advantages of the new instrument are evident. With such a series of levers, however, there must be a very fine adjustment and consequently some danger of derangement. With the addition of means for estimating the force of the movement the instrument would be well-nigh perfect. However, as it is, it will be of great value, and doubtless will be the means of directing more workers to this interesting field. The instrument may be ordered through the Psychiatric Klinik at Giessen. The price is not given.

SHEPHERD IVORY FRANZ.

COLUMBIA UNIVERSITY.

Growth of Children in Height and Weight. FREDERICK BURK. Am. Jour. Psy., IX., 3, April, 1898.

This is not so much an original investigation into the subject indicated, as a careful study and correlation of the investigations of others. The writer's object seems to be to discover what bearing the extant studies in child-growth have upon the problems of Pedagogy. Extensive citations are made from Bowditch, Camerer, Combe, Hertel, Hansen, Key, Porter and many others; and the following are some of the conclusions drawn or suggested:

Growth, both in height and weight, is far more rapid during the first year than at any other time. Then follows a constant though rhythmical decrease in the rate of growth until the pubertal age, during which a very marked acceleration takes place, culminating in the virtual attainment of maturity, so far as height is concerned, in the 15th or 16th year for girls, and a year or two later for boys. The attainment of maturity in weight is, of course, considerably later than this for both sexes. The average boy is 19.68 inches in height at birth, about 29 inches at the end of his first year, 45 inches in his 7th year, and 55 inches in his 12th year. There is but little difference in the rate of growth of the two sexes until about the tenth year, when girls

on account of the earlier advent of their pubertal acceleration, begin to grow more rapidly than boys, and for the next two or three years are actually taller. Accelerated growth in boys begins at 12 to 14, and is usually completed in the 16th year. The average weight at birth is 7.3 pounds for boys and 7.1 pounds for girls. Development in weight follows nearly the same laws as development in stature. Autumn is the best season of the year for growth in weight, spring and summer for stature. Children in comfortable circumstances grow larger than those reared in poverty, but it is by no means certain that mental growth corresponds with physical, and it is an open question whether the period of most rapid physical growth is also the time most appropriate for close mental application. The age of puberty is found to be the time of greatest resistance to disease. All growth is rhythmical, not only in the ordinary sense, but also in the sense that some parts of the body forge ahead at certain times, other parts at other times; a fact which suggests to the writer the idea of a principle of compensation in physical development.

The article suggests not only the extent of what has been done in the field of anthropometry, but the far greater extent of what remains to be done, as well as the complicated nature of many of the problems involved. In the case of some of these problems it is exceedingly doubtful whether any result applicable to pedagogy is likely to be attained.

F. TRACY.

UNIVERSITY OF TORONTO.

Psychology and Physiology. HERBERT NICHOLS. American Journal of Insanity, October, 1897. Pp. 183–200.

This article is a lecture delivered at Johns Hopkins University in March, 1896, one of six on 'Modern Psychology and its Bearings.' Dr. Nichols states that it is his present purpose to point out that although psychology and physiology are close together and even overlapping in some ways, *e. g.*, in subject-matter, yet in other respects the two sciences are too widely apart—"in points of view, in practical touch and in working support." He suggests his notion how intimately connected the two sciences are by noting that out of the 1,556 pages of the last edition of 'Foster's Physiology' 772 pages are devoted to the muscles, nerves, and sense organs, and by the opinion that "every word of this must now be considered the indispensable property alike of both sciences." None have been in the past more active than physiologists in removing from their science the false notions of scholasticism and of mysticism, but there seems to be danger now that

they will improperly refuse to admit the doctrines of modern psychology, as if they were still on the old and now outworn basis. The harm which may come from attempting to tie down modern physiology to the crude notions of the old psychology may be seen from the hindrance and confusion which such conservatism has already caused in the scientific study of such related subjects, for example, as visceral sensations, emotions, instincts, subconscious processes, and personality. Dr. Nichols then elaborates somewhat these examples in the line of his theory of pain sensation, and according to the prevalent doctrines as to cœnesthesia, subconsciousness, and instinct.

These subjects, and especially personality, physiology seems to dread to investigate, lest it be thought to lay itself liable to the once more reasonable reproach that it is metaphysically speculative. In closing, Dr. Nichols expresses the following very pertinent opinion: "There is as much need that physiology shall unite now, heartily and generously, with psychology to purge the realm of the latter from primitive scholasticism, as it was, at an earlier date, for her to drive out from her own realm the false notions of physiology derived in the same manner from the same sources."

GEORGE V. DEARBORN.

Animal Intelligence: An Experimental Study of the Associative Processes in Animals. By E. L. THORNDIKE. Monograph Supplement No. 8 of the PSYCHOLOGICAL REVIEW.

In this monograph are presented the results of some experiments which I have been carrying on during two years, and some theories which these results seem to support. The subjects of the experiments were dogs, cats and chicks, and the method was to put them, when hungry, in boxes from which they could escape and so get food by manipulating some simple mechanism (*e. g.*, by pulling down a loop of wire, depressing a lever, turning a button). On the first occasion that a young animal is put into such a box, it reacts instinctively with attempts to squeeze through holes, to claw down bars, to bite through the confining walls, to claw at things outside the box as if to pull itself through. If the animal does not, in the course of such activity, hit upon the proper act by chance, it, of course, never forms the association between the sight of the interior of that box and a certain act. But if it does hit upon the proper act, the resulting pleasure stamps in that fragment of its mass of instinctive impulses and renders it more likely to come up in connection with the sense-impression of that box. The failure of all the other acts to win pleasure stamps them out.

After repeated trials, therefore, the animal will perform the act immediately on being confronted with the situation. Thus the successive times taken by one cat in a certain box were (in seconds) 160, 30, 90, 60, 15, 28, 20, 30, 22, 11, 15, 20, 12, 10, 14, 8, 8, 5, 10, 8, 6, 6, 7. Such time records were kept in all the experiments.

The first result of the work is an accurate idea of just what things a cat or dog or chick can learn, what things are easy, and how long each association is in forming. The next result is a final disproof of the theory that the acts of animals involve reasoning, comparison, inference. The disproof consists in the fact that my animals did do by chance and learn by association, representative acts (*e. g.*, using thumb-latch and button) of those which have been fancied to require reason. Further, they would, in the case of some difficult associations, happen to do the thing six or seven times, but after long periods of promiscuous scrabbling, and then forever after would fail to do it. If they had acted from inference in any case they ought not to have failed in the seventh or eighth trial. What had been inferred six times should have been inferred the seventh. Finally, in all associations, the decrease in the time taken is *gradual*. Even after doing the thing the animal does not know enough to realize what it has done and thereafter do it as soon as put in the box.

Experiments on imitation showed that these animals could not learn to do the simplest acts from seeing their fellows do them. Unless observation of the Primates has been completely mistaken, we get here a clear differentiation and advance.

The most important experiments theoretically were a series which showed that the animals could not learn to do any act from being put through it and that no association leading to an act could be formed unless there was included in the association an impulse of the animal's own. The animals, that is, did not associate the *idea* of *being in* the box with the *idea* of pulling the loop and with the *idea* of eating fish outside and supply the impulse to *go in*.

These phenomena, supported by general observation of the animals' behavior, lead one to suppose, as one great difference between man and the brutes, the possession of a stock of free ideas and impulses. The animal's intellectual conciousness consists of a lot of specific and, for the most part, direct connections between sense-impressions and ideas and impulses to action. Association in animals is not homologous with anything in human association except such conscious connections as a man feels in playing tennis or billiards. The essential thing in it is not the idea, but the impulse. That this sort of human associations

is homologous with the animal sort is borne out by the fact that they are, like the latter, formed *gradually* by the stamping in of successes due to trial and error, and are not able to be formed by imitation or by one's being put through them. An attempt is made to explain the origin of the distinctively human sort and to show how the stock of free ideas is a requisite for the development of higher mental processes.

Quantitative estimates of the delicacy, complexity, number and permanence of associations in these animals were derived from further experiments. The important theoretical consideration gained is that the animals possessed no memory, no realization of a situation as the same that had been encountered in the past. Their memory is only the memory of the billiard player who plays after an interval without practice.

The author's conception of mental evolution is briefly explained, and applications of his results to education, anthropology and theoretical psychology are made.

THE AUTHOR.

NEW BOOKS.

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L'Année philosophique. F. PILLON. Paris, Alcan. 1898. 5 fr.

The Metaphysic of Experience. Vol. II. Positive Science. SHADWORTH H. HODGSON. London, New York and Bombay, Longmans, Green & Co. 1898. Pp. viii + 403.

The Nature and Development of Animal Intelligence. WESLEY MILLS. New York, The Macmillan Company. 1898. Pp. xii + 307. $2.00.

Psychology for Teachers. C. LLOYD MORGAN, with a preface by HENRY W. JAMESON. New York, Scribner's. 1898. Pp. xi+236. $1.00.

The Gospel according to Darwin. WOODS HUTCHINSON. Chicago, The Open Court Publishing Co. London, Kegan Paul. 1898. Pp. viii+240. $1.50.

Aphasia and other Speech Defects. H. C. BASTIAN. London, Lewis. 1898. Pp. ix+314.

Psychologie du peuple français. ALFRED FOUILLÉE. Paris, Alcan. 1898. Pp. iv+388.

Immanuel Kant, sein Leben und seine Lehre. F. PAULSEN. Stuttgart, Frommanns. 1898. Pp. xii+396.

Les origines de la psychologie contemporaine. D. MERCIER. Paris, Alcan. 1898. 5 fr.

Contribution à l'étude de l'hérédité et des principes de la formation des races. J. M. HARRACA. Paris, Alcan. 1898. 2 fr.

NOTES.

PROFESSOR D. G. RITCHIE, of St. Andrews' University, has been elected president of the Aristotelean Society for the ensuing year.

IN the absence of Mr. W. H. R. Rivers, who has arrived safely with Professor Haddon's expedition at Murray Island, courses in experimental psychology in University College, London, will be given by Mr. E. T. Dickson.

PROFESSOR W. LE CONTE STEVENS, known for his contributions to physiological optics, has accepted the chair of physics in Washington and Lee University.

A SUMMER meeting of the American Psychological Association in connection with the American Association for the Advancement of Science, was given up owing to the small number of papers offered. Several papers of psychological interest were presented before the Section of Anthropology and other sections of the Association.